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PROCEEDINGS

OF THE

ROYAL SOCIETY OF LONDON.

VOL. LXXI.

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JUNE, 1903.

LONDON :

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MINUTES FOR SESSION 1902—1903.

November 20, 1902.

Dr. W. T. BLANFORD, Vice-President, in the Chair.

Mr. H. Brereton Baker was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

The Rev. Robert Harley, Professor Meldola, and Dr. R. H. Scott were by ballot elected Auditors of the Treasurer's accounts on the part of the Society.

The following Papers, received during the Recess, and published in full or in abstract, in accordance with the Standing Orders of Council, were read in title:—

"Observations on 'Flicker' in Binocular Vision." By C. S. SHERRINGTON, M.A., M.D., F.R.S.

"Harmonic Tidal Constants for certain Australian and Chinese Ports." By THOMAS WRIGHT, of the Nautical Almanac Office. Communicated by Professor G. H. DARWIN, F.R.S.

"On the Influence of the Prolonged Action of the Temperature of Liquid Air on Micro-organisms, and on the Effect of Mechanical Trituration at the Temperature of Liquid Air on Photogenic Bacteria." By ALLAN MACFADYEN, M.D. Communicated by Professor JAMES DEWAR, F.R.S.

"On the Measurement of the Bactericidal Power of small Samples of Blood under Aerobic and Anaerobic Conditions, and on the Comparative Bactericidal Effect of Human Blood drawn off and tested under these Contrasted Conditions." By A. E. WRIGHT, M.D., Professor of Pathology, Army Medical School, Netley. Communicated by Professor J. R. BRADFORD, F.R.S.

"On Changes in Elastic Properties produced by the sudden Cooling or 'Quenching' of Metals." By JAMES MUIR, B.A., D.Sc. Communicated by Professor J. A. EWING, F.R.S.

'The Fracture of Metals under repeated Alternations of Stress." By J. A. Ewing, LL.D., F.R.S., and J. C. W. Humfrey, B.A., St. John's College, Cambridge.

"An Intracellular Toxin of the Typhoid Bacillus." By Allan Macfadyen, M.D., and Sydney Rowland, M.A. Communicated by Lord Lister, F.R.S.

The following Papers were read:—

I. "Report on the recent Eruption of the Soufrière, in St. Vincent, and of a Visit to Mont Pelée. Part I." By Tempest Anderson, M.D., B.Sc., F.G.S., and John S. Flett, M.A., D.Sc., F.G.S. Communicated by the Secretaries of the Royal Society.

II. "On the Correlation of the Mental and Physical Characters in Man. Part II." By Alice Lee, D.Sc., Marie A. Lewenz, B.A., and Karl Pearson, F.R.S.

III. "Contributions to a Theory of the Capillary Electrometer. II.—On an Improved Form of Instrument." By G. J. Burch, M.A. Oxon., F.R.S.

IV. "An Experimental Determination of the Variation of the Critical

President.—Sir William Huggins, K.C.B., O.M., D.C.L., LL.D.

Treasurer.—Alfred Bray Kempe, M.A.

Secretaries.— { Sir Michael Foster, K.C.B., D.C.L., LL.D.
Joseph Larmor, M.A., D.Sc., LL.D.

Foreign Secretary—Thomas Edward Thorpe, C.B., Sc.D.

Other Members of the Council.—William Bateson, M.A.; William Thomas Blanford, LL.D.; Professor Hugh Longbourne Callendar, M.A., LL.D.; Francis Darwin, M.A.; Professor Harold Baily Dixon, M.A.; Professor George Carey Foster, LL.D.; Right Hon. Sir John E. Gorst, M.A.; Professor John Wesley Judd, C.B., LL.D.; Right Hon. the Lord Lister, O.M., F.R.C.S.; Professor George Downing Liveing, M.A.; Professor Augustus Edward Hough Love, M.A.; Professor Henry Alexander Miers, M.A.; Professor Edward Albert Schäfer, LL.D.; Captain Thomas Henry Tizard, R.N., C.B.; Professor Herbert Hall Turner, M.A.; and Sir John Wolfe Barry, K.C.B., LL.D.

The following Papers were read:—

I. "Experiments on the Effect of Mineral Starvation on the Parasitism of the Uredine Fungus *Puccinia dispersa* on Species of *Bromus.*" By Professor H. MARSHALL WARD, F.R.S.

II. "Note upon Descending Intrinsic Spinal Tracts in the Mammalian Spinal Cord." By Professor C. S. SHERRINGTON, F.R.S., and Dr. E. E. LASLETT.

III. "The Inter-relationship of Variola and Vaccinia." By Dr. S. MONCKTON COPEMAN. Communicated by LORD LISTER, F.R.S.

IV. "The Colour-physiology of the Higher Crustacea." By F. KEEBLE and F. W. GAMBLE. Communicated by Professor S. J. HICKSON, F.R.S.

December 1, 1902.

Anniversary Meeting.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

The Report of the Auditors of the Treasurer's accounts was read, and the thanks of the Society were given to the Treasurer and to the Auditors.

The List of Fellows deceased and Fellows elected into the Society since the last Anniversary was read.

The Report to the Society from the Council upon their work during the past year was, upon the motion of the President, received.

The President delivered his Anniversary Address, and, on the motion of Sir John Evans, seconded by Dr. Sclater, the thanks of the Society were given to the President for his Address, and he was requested to allow it to be printed.

The Awards of the Medals for the year were announced as follows, and the Medals were presented from the Chair :—

The Copley Medal......	To Lord Lister, F.R.S.
The Rumford Medal ...	,, the Hon. Charles A. Parsons, F.R.S.
A Royal Medal	,, Prof. Horace Lamb, F.R.S.
A Royal Medal	,, Prof. Edward A. Schäfer, F.R.S.
The Davy Medal	,, Prof. Svante August Arrhenius.
The Darwin Medal	,, Mr. Francis Galton, F.R.S.
The Buchanan Medal ..	,, Dr. Sydney A. Monckton Copeman.
The Hughes Medal	,, Prof. Joseph John Thomson, F.R.S.

The President having, with the consent of the Society, nominated Dr. Günther and Major MacMahon as scrutators to assist the Secretaries in examining the balloting lists for the election of Council and Officers, *December* 4, 1902.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

The President announced that he had appointed as Vice-Presidents for the ensuing year—

The Treasurer.
Dr. Blanford.
Professor G. Carey Foster.
Professor Judd.

The following Papers were read:—

I. "On the 'Blaze Currents' of the Incubated Hen's Egg." By Dr. A. D. WALLER, F.R.S.

II. "On the 'Blaze Currents' of the Crystalline Lens." By Dr. A. D. WALLER, F.R.S., and A. M. WALLER.

III. "A Contribution to the Question of 'Blaze Currents.'" By Dr. A. DURIG. Communicated by Dr. A. D. WALLER, F.R.S.

IV. "On the Similarity of the Short-period Pressure Variation over Large Areas." By Sir NORMAN LOCKYER, F.R.S., and Dr. W. J. S. LOCKYER.

V. "Isomeric Change in Benzene Derivatives.—The Interchange of Halogen and Hydroxyl in Benzenediazonium Hydroxides." By Dr. K. J. P. ORTON. Communicated by Professor ARMSTRONG, F.R.S.

VI. "On the Vibrations and Stability of a Gravitating Planet." By J. H. JEANS. Communicated by Professor G. H. DARWIN, F.R.S.

December 11, 1902.

SIR WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

Mr. Hugh Frank Newall was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On certain Properties of the Alloys of the Gold-Silver Series." By the late Sir Wm. Roberts-Austen, K.C.B., F.R.S., and Dr. T. Kirke Rose.

II. "The Spectrum of γ Cygni." By Sir Norman Lockyer, K.C.B., F.R.S., and F. E. Baxandall.

III. "Abnormal Changes in some Lines in the Spectrum of Lithium." By Hugh Ramage, B.A. Communicated by Professor Liveing, F.R.S.

IV. "Quaternions and Projective Geometry." By Professor C. J. Joly, F.T.C.D. Communicated by Sir Robert Ball, F.R.S.

V. "An Error in the Estimation of the Specific Gravity of the Blood by Hammerschlag's Method, when employed in connection with Hydrometers." By Dr. A. G. Levy. Communicated by Sir Victor Horsley, F.R.S.

VI. "The Specific Heats of Metals and the Relation of Specific Heat to Atomic Weight. Part II." By Professor W. A. Tilden, F.R.S.

The Society adjourned over the Christmas Recess to Thursday, January 22, 1903.

January 22, 1903.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

The Right Hon. Horace Plunkett, a Member of His Majesty's Most Honourable Privy Council, was admitted into the Society.

A List of the Presents was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Preliminary Note on the Relationships between Sun-spots and Terrestrial Magnetism." By C. CHREE, M.A., Sc.D., F.R.S.

II. "Characteristics of Electric Earth-current Disturbances, and their Origin." By J. E. TAYLOR. Communicated by Sir OLIVER LODGE, F.R.S.

III. "Solar Eclipse of 1900, May 28th.—General Discussion of Spectroscopic Results." By J. EVERSHED, F.R.A.S. Communicated by the Joint Permanent Eclipse Committee.

IV. "Some Dielectric Properties of Solid Glycerine." By Professor ERNEST WILSON. Communicated by Sir WILLIAM PREECE, K.C.B., F.R.S.

V. "On the Electrodynamic and Thermal Relations of Energy of Magnetisation." By Dr. J. LARMOR, Sec. R.S.

January 29, 1903.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

The Right Hon. Lord Alverstone, Lord Chief Justice, was admitted into the Society.

A List of the Presents was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "The Relation between Solar Prominences and Terrestrial Magnetism." By Sir NORMAN LOCKYER, K.C.B., F.R.S., and Dr. W. J. S. LOCKYER.

II. "The Bending of Electric Waves round a Conducting Obstacl
By H. M. MACDONALD, M.A., F.R.S.

III. "On Skew Refraction through a Lens; and on the Hollow Per
given by an Annulus of a very obliquely placed Lens."
J. D. EVERETT, M.A., D.C.L., F.R.S.

February 12, 1903.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

A List of the Presents was laid on the table, and thanks ordered for them.

The President made reference to the great loss which the Society had sustained through the death of Sir George Gabriel Stokes, Past-President, and read the following letter, which had been drawn up by the Council for dispatch to Sir Arthur Stokes, Bart.:—

"*Royal Society,*
"*Burlington House, W.*,
"*February* 12, 1903.

"DEAR SIR ARTHUR,

"We are desired by the President and Council of the Royal Society, and by the Fellows assembled at the ordinary meeting held this day, to make known to you, and to your sister, Mrs. L. Humphry, their most sincere sympathy in the great loss which has fallen upon you through the death of your illustrious father.

"For the long period of thirty-one years the Society was greatly strengthened by the presence of your father as one of its secretaries. Throughout this period his labours were unremitting, both in the discharge of his official duties and in assistance given to the work of individual Fellows; they were thus most fruitful for the progress of science. For five years the Society was proud to claim him as its President.

"Among the great names which are to be found in the roll of the Society, your father's will always be held in honour among the greatest.

"It may be some little consolation to you in your present great grief to be told how much his brethren in science admired, and while admiring, loved him.

"We are,
"Yours very truly,
"(Sd.) M. FOSTER,
"J. LARMOR,
"*Secretaries R.S.*

"*Sir Arthur R. Stokes, Bart.*"

The following Papers were read:—

I. "On the Decline of the Injury Current in Mammalian Nerve, and its Modification by Changes of Temperature.—Preliminary Communication." By Miss S. C. M. SOWTON and J. S. MACDONALD. Communicated by Professor SHERRINGTON, F.R.S.

II. "On the Negative Variation in the Nerves of Warm-blooded Animals." By Dr. N. H. ALCOCK. Communicated by Dr. WALLER, F.R.S.

III. "On the Optical Activity of Hæmoglobin and Globin." By Professor GAMGEE, F.R.S., and A. CROFT HILL.

IV. "On the Nucleo-proteids of the Pancreas, Thymus, and Suprarenal Gland, with especial Reference to their Optical Activity." By Professor GAMGEE, F.R.S., and Professor W. JONES.

V. "Studies in the Morphology of Spore-producing Members. No. V.—General Comparisons and Conclusion." By Professor F. O. BOWER, F.R.S.

VI. "Primitive Knot and Early Gastrulation Cavity co-existing with Independent Primitive Streak in *Ornithorhynchus*." By Professor J. T. WILSON and J. P. HILL. Communicated by Professor HOWES, F.R.S.

February 26, 1903.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

A List of the Presents was laid on the table, and thanks ordered for them.

The Bakerian Lecture, "On the Constitution of the Copper-Tin Series of Alloys," was delivered by C. T. HEYCOCK, F.R.S., and F. H NEVILLE, F.R.S.

March 5, 1903.

Professor J. W. JUDD, C.B., Vice-President, in the Chair.

Professor Gösta Mittag-Leffler, For. Mem. R.S. (elected 1896) was admitted into the Society.

A List of the Presents was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of Candidates for election into the Society were read as follows:—

Adami, John George.
Allen, Alfred Henry.
Ardagh, Major-General Sir John.
Ballance, Charles Alfred.
Bather, Francis Arthur.
Bayliss, William Maddock.
Biles, Professor John Harvard.
Binnie, Sir Alexander Richardson.
Bridge, Professor Thomas William.
Brodie, Thomas Gregor.
Bruce, John Mitchell.
Budge, Ernest A. Wallis.
Burrard, Sidney Gerald.
Callaway, Charles.
Carbutt, Sir Edward H.
Cardew, Major Philip.
Chattaway, Frederick Daniel.
Chattock, Arthur Prince.
Clowes, Frank.
Copeman, Sydney Monckton.
Corfield, Professor William Henry.
Crompton, Rookes Evelyn B.
Crookshank, Professor Edgar March.
Darwin, Horace.
Davison, Charles.
Dendy, Professor Arthur.
Dines, William Henry.
Dobbie, Professor James Johnstone.
Durston, Sir Albert John.
Garrod, Archibald Edward.
Goodrich, Edwin S.
Gray, Professor Thomas.

Harcourt, Leveson Francis Vernon.
Harmer, Frederic William.
Hiern, William Philip.
Hills, Major Edmund Herbert.
Hopkins, Frederick Gowland.
Hopkinson, Edward.
Jukes-Browne, Alfred John.
Knott, Cargill Gilston.
Lees, Charles H.
Letts, Professor Edmund Albert.
Lewis, Sir William Thomas.
MacArthur, John Stewart.
Maclean, Magnus.
MacMunn, Charles Alexander.
Major, Charles I. Forsyth.
Mallock, Henry Reginald Arnulph.
Mance, Sir Henry C.
Marsh, James Ernest.
Masson, Professor Orme.
Matthey, Edward.
Maunder, Edward Walter.
Plimmer, Henry G.
Prain, Major David.
Ridley, Henry Nicholas.
Rose, Thomas Kirke.
Russell, James Samuel Risien.
Rutherford, Professor Ernest
Sampson, Professor Ralph Allen.
Sclater, William Lutley.
Searle, George C. F.
Sharpe, R. Bowdler.
Shipley, Arthur Everett.
Sidgreaves, Rev. Walter.
Smith, Professor Grafton Elliot.
Smith, James Lorrain.
Stead, John Edward.
Strahan, Aubrey.
Swinburne, James.
Swinton, Alan Archibald Campbell.
Symington, Professor Johnson.
Tarleton, Professor Francis Alexander.

IV. "On the Laws governing Electric Discharges in Gases at Low Pressures." By W. R. CARR. Communicated by Professor J. J. THOMSON, F.R.S.

V. "The Differential Invariants of a Surface, and their Geometric Significance." By Professor A. R. FORSYTH, F.R.S.

March 12, 1903.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, followed by Professor J. W. JUDD, C.B., Vice-President, in the Chair.

A List of the Presents was laid on the table, and thanks ordered for them.

The following papers were read:—

I. "On the Histology of *Uredo dispersa*, Erikss., and the 'Mycoplasm' Hypothesis." By Professor MARSHALL WARD, F.R.S.

II. "The Statolith-theory of Geotropism." By F. DARWIN, F.R.S.

III. "A Study of a Unicellular Green Alga, occurring in Polluted Water, with especial reference to its Nitrogenous Metabolism." By Miss H. CHICK. Communicated by Professor R. BOYCE, F.R.S.

IV. "A Comparative Study of the Grey and White Matter of the Motor-cell Groups and of the Spinal Accessory Nerve in the Spinal Cord of the Porpoise (*Phocæna communis*)." By Dr. D. HEPBURN and Dr. D. WATERSTON. Communicated by Sir WILLIAM TURNER, F.R.S.

V. "The Œstrous Cycle and the Formation of the Corpus Luteum in the Sheep." By F. H. A. MARSHALL. Communicated by Professor J. C. EWART, F.R.S.

VI. "On the Culture of the Nitroso-bacterium." By H. S. FREMLIN. Communicated by Sir MICHAEL FOSTER, Sec. R.S.

VII. "Upon the Immunising Effects of the Intracellular Contents of the Typhoid Bacillus as obtained by the Disintegration of the Organism at the Temperature of Liquid Air." By Dr. A. MACFADYEN. Communicated by Lord LISTER, O.M., F.R.S.

March 19, 1903.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

A List of the Presents was laid on the table, and thanks ordered for them.

The following papers were read :—

I. "On the Formation of Barrier Reefs and of the Different Types of Atolls." By Professor Alex. Agassiz, For. Mem. R.S.

II. "On Central American Earthquakes, particularly the Earthquakes of 1838." By Admiral Sir John Dalrymple Hay, F.R.S.

III. "The Emanations of Radium." By Sir William Crookes, F.R.S.

VI. "On the Dependence of the Refractive Index of Gases on Temperature." By G. W. Walker. Communicated by Professor J. J. Thomson, F.R.S.

VII. "Solar Prominence and Spot Circulation, 1872—1901." By Sir Norman Lockyer, F.R.S., and Dr. W. J. S. Lockyer.

VIII. "On the Evolution of the Proboscidea." By Dr. C. W. Andrews. Communicated by Professor E. Ray Lankester, F.R.S.

IX. "On the Cytology of Apogamy and Apospory.—I. Preliminary Note on Apogamy." By Professor J. Bretland Farmer, F.R.S., J. G. S. Moore, and Miss L. Digby.

The Society adjourned over the Easter Recess to Thursday, April 30.

II. " On the Physiological Action of the Poison of the Hydrophidæ. By Dr. Leonard Rogers. Communicated by Major A. Alcock, F.R.S.

III. " Preliminary Note on the Discovery of a Pigmy Elephant in the Pleistocene of Cyprus." By Miss D. M. A. Bate. Communicated by Dr. Henry Woodward, F.R.S.

IV " Experiments in Hybridisation, with Special Reference to the Effect of Conditions on Dominance." By L. Doncaster, B.A. Communicated by Dr. S. F. Harmer, F.R.S.

April 30, 1903.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

A List of the Presents received was laid on the table, and thanks lered for them.

The Croonian Lecture: "The Cosmical Function of the Green ant," was delivered by Professor TIMIRIAZEFF, of the University of oscow.

The following paper was read:—

"Preliminary Note on the Use of Chloroform in the Preparation of Vaccine." By ALAN B. GREEN, M.A., M.D. Communicated by W. H. POWER, C.B., F.R.S.

May 14, 1903.

Professor CAREY FOSTER, Vice-President, in the Chair.

A list of the Presents received was laid on the table, and thanks ordered for them.

The following papers were read:—

I. "On the Discovery of a Species of Trypanosoma in the Cerebro-spinal Fluid of Cases of Sleeping Sickness." By Dr. ALDO CASTELLANI. Communicated by the Malaria Committee of the Royal Society.

II. "The Combination of Hydrogen and Chlorine under the Influence of Light." By P. V. BEVAN. Communicated by Professor J. J. THOMSON, F.R.S.

III. "On the Photo-Electric Discharge from Metallic Surfaces in Different Gases." By Dr. W. MANSERGH VARLEY. Communicated by Professor J. J. THOMSON, F.R.S.

IV. "The Elasmometer, a New Interferential Form of Elasticity Apparatus." By A. E. TUTTON, F.R.S.

V. "Meteorological Observations by the Use of Kites off the West Coast of Scotland, 1902." By W. N. SHAW, F.R.S., and W. H. DINES, F.R. Met. Soc.

VI. "On the Radiation of Helium and Mercury in a Magnetic Field." By Professor ANDREW GRAY, F.R.S., and Dr. W. STEWART, with R. A. HOUSTOUN and D. B. MCQUISTAN.

VII. "A New Class of Organo-Tin Compounds containing Halogens." By Professor W. J. POPE, F.R.S., and S. J. PEACHEY.

VIII. "The Xanthophyll Group of Yellow Colouring Matters." By C. A. SCHUNCK. Communicated by H. T. BROWN, F.R.S.

May 28, 1903.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

A List of the Presents was laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Bending of Waves round a Spherical Obstacle." By LORD RAYLEIGH, O.M., F.R.S.

II. "Sur la Diffraction des Ondes Electriques, à propos d'un Article de M. Macdonald." By Professor H. POINCARÉ, For. Mem. R.S.

III. "On the Theory of Refraction in Gases." By G. W. WALKER, M.A. Communicated by Professor J. J. THOMSON, F.R.S.

IV. "An Analysis of the Results from the Kew Magnetographs on *Quiet* Days during the Eleven Years 1890 to 1900, with a Discussion of certain Phenomena in the Absolute Observations." By Dr. C. CHREE, F.R.S.

V. "On a Remarkable Effect produced by the Momentary Relief of Great Pressure." By J. Y. BUCHANAN, F.R.S.

VI. "Evolution of the Colour-pattern and Orthogenetic Variation in certain Mexican Species of Lizards with Adaptation to their Surroundings." By Dr. H. GADOW, F.R.S.

VII. "Researches on Tetanus—Preliminary Communication." By Professor HANS MEYER and Dr. F. RANSOM. Communicated by Professor E. H. STARLING, F.R.S.

VIII. "The Hydrolysis of Fats *in vitro* by Means of Steapsin." By Dr. J. LEWKOWITSCH and Dr. J. J. R. MACLEOD. Communicated by Professor E. DIVERS, F.R.S.

IX. "On the Optical Activity of the Nucleic Acid of the Thymus Gland." By Professor A. GAMGEE, F.R.S., and Dr. W. JONES.

X. "Note on the Effect of Extreme Cold on the Emanations of Radium." By Sir W. CROOKES, F.R.S., and Professor J. DEWAR, F.R.S.

XI "On the Adaptation of the Pancreas to Different Food-stuffs—Preliminary Communication." By F. A. BAINBRIDGE, M.B. Communicated by Professor STARLING, F.R.S.

The Society adjourned over the Whitsuntide Recess to Thursday, June 11.

June 11, 1903.

Annual Meeting for the Election of Fellows.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

The Statutes relating to the Election of Fellows having been read, Major MacMahon and Dr. A. D. Waller were, with the consent of the Society, nominated Scrutators, to assist the Secretaries in the examination of the balloting lists.

The following Papers were read:—

I. "The Bending of Electric Waves round a Conducting Obstacle: Amended Result." By H. M. MACDONALD, F.R.S.

II. "On the Propagation of Tremors over the Surface of an Elastic Solid." By Professor H. LAMB, F.R.S.

III. "The Diffusion of Salts in Aqueous Solutions." By J. C. GRAHAM. Communicated by Professor W. E. AYRTON, F.R.S.

IV. "On the Structure of Gold Leaf, and the Absorption Spectrum of Gold." By Professor J. W. MALLET, F.R.S.

V. "On Reptilian Remains from the Trias of Elgin." By G. A. BOULENGER, F.R.S.

VI. "A Method for the Investigation of Fossils by Serial Sections." By Professor W. J. SOLLAS, F.R.S.

VII. "An Account of the Devonian Fish, *Palæospondylus Gunni*, Traquair." By Professor W. J. SOLLAS, F.R.S., and Miss I. B. J. SOLLAS.

VIII. "The Measurements of Tissue Fluid in Man. Preliminary Note." By Dr. G. OLIVER Communicated by Sir LAUDER BRUNTON, F.R.S.

IX. "Observations on the Physiology of the Cerebral Cortex of the Anthropoid Apes." By Dr. A. S. F. GRÜNBAUM and Professor C. S. SHERRINGTON, F.R.S.

June 18, 1903.

Sir WILLIAM HUGGINS, K.C.B., O.M., President, in the Chair.

Dr. William Maddock Bayliss, Professor Thomas William Bridge, Dr. Sydney Monckton Copeman, Mr. Horace Darwin, Mr. William Philip Hiern, Mr. Henry R. A. Mallock, Mr. Arthur George Perkin, Mr. John Edward Stead, and Mr. Aubrey Strahan were admitted into the Society.

A List of the Presents received was laid on the table, and thanks dered for them.

The following Papers were read :—

I. "Surface Flow in Crystalline Solids under Mechanical Disturbance." By G. T. Beilby. Communicated by F. H. Neville, F.R.S.

II. "The Effects of Heat and of Solvents on Thin Films of Metal." By G. T. Beilby. Communicated by F. H. Neville, F.R.S.

III. "The Forces acting on a Charged Electric Condenser moving through Space." By Professor Trouton, F.R.S., and H. R. Noble.

IV. "On the Discharge of Electricity from Hot Platinum." By Dr. H. A. Wilson. Communicated by C. T. R. Wilson, F.R.S.

V. "The Bionomics of *Convoluta Roscoffensis*, with Special Reference to its Green Cells." By Dr. F. W. Gamble and F. W. Keeble. Communicated by Professor S. J Hickson, F.R.S.

XII. "The Phenomena of Luminosity and their Possible Correlation with Radio-Activity." By Professor H. E. Armstrong, F.R.S., and Dr. T. Martin Lowry.

XIII. "Cyanogenesis in Plants. Part III.—On Phaseolunatin, the Cyanogenetic Glucoside of Phaseolunatus." By Professor W. R. Dunstan, F.R.S., and Dr. T. A. Henry.

XIV. "The Magnetic Expansion of some of the Less Magnetic Metals." By Dr. P. E. Shaw. (With an Appendix by G. A. Schott.) Communicated by Professor Poynting, F.R.S.

XV. "A Study of the Interaction of Mercury and Nitric Acid." By Professor Chandra Rây. Communicated by Sir Henry Roscoe, F.R.S.

XVI. "Separation of Solids in the Surface-layers of Solutions and Suspensions." By Dr. W. Ramsden. Communicated by Professor F. Gotch, F.R.S.

XVII. "Some Preliminary Observations on the Assimilation of Carbon Monoxide by Green Plants." By Professor W. B. Bottomley and Professor Herbert Jackson. Communicated by Professor Reynolds Green, F.R.S.

XVIII. "On the Oocyte of Tomopteris." By W. Wallace. Communicated by Professor McIntosh, F.R.S.

XIX. "Upon the Bactericidal Action of some Ultra-violet Radiations as produced by the Continuous Current Arc." By J. E. Barnard and H. de R. Morgan. Communicated by Sir Henry Roscoe, F.R.S.

XX. "The Longitudinal Stability of Aerial Gliders." By Professor G. H. Bryan, F.R.S., and W. E. Williams.

XXI. "On the Synthesis of Fats accompanying Absorption from the Intestine." By Professor B. Moore. Communicated by Professor Sherrington, F.R.S.

XXII. "Radiation in the Solar System.—Its Effect on Temperature and its Pressure on Small Bodies." By Professor J. H. Poynting, F.R.S.

XXIII. "The Properties of Aluminium-Tin Alloys." By Dr. W. Carrick Anderson and G. Lean. Communicated by Professor Miers, F.R.S.

XXIV. "The 'Hunting' of Alternating-Current Machines." By BERTRAM HOPKINSON. Communicated by Professor EWING, F.R.S.

XXV. "The Theory of Symmetrical Optical Objectives." By S. D. CHALMERS. Communicated by Professor LARMOR, Sec. R.S.

XXVI. "The Differential Invariants of Space." By Professor A. R. FORSYTH, F.R.S.

The Society adjourned over the Long Vacation to Thursday, ovember 19.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

"Magnetic Observations in Egypt, 1893—1901." By Captain H. G. LYONS, R.E. Communicated by Professor RÜCKER, Sec. R.S. Received June 6,—Read June 20, 1901.

The following Magnetic Observations have been made at various times during the years 1893 to 1899, at first with a Declinatorium, made by Bamberg, of Berlin, the property of the Egyptian Government, and later with a Kew Magnetometer, No. 73, and Dover's Dip Circle, No. 99, both kindly lent by the Council of the Royal Society on the recommendation of Professor A. W. Rücker, F.R.S. These observations are most conveniently divided into five groups, each of which includes observations made during a single period and with a single instrument—

I. Observations made with a Declinatorium by Bamberg, of Berlin:

(*a.*) In the neighbourhood of Cairo, 1893—1894.
(*b.*) In the Lybian Desert, near the Kharga and Dakhla Oases, in December, 1893, and January, 1894.
(*c.*) In the Lybian Desert from the Wadi Natrun to the Baharia Oasis, April, 1894.

II. Observations taken with Kew Magnetometer, No. 73, and Dover's Dip Circle, No. 99, in the Nile Valley from Cairo to the 2nd Cataract, November, 1894, to June, 1896.

(*a.*) Declination. (*b.*) Dip and Horizontal Force.

III. Observations taken to determine the Diurnal Variation of the Declination.

IV. Observations taken at Helwan, near Cairo, in November and December, 1898.

V. Determination of the Annual Variation from the above observations and those of various observers in previous years.

The declinatorium used at first consisted of a horizontal circle furnished with two verniers reading to 30″, while the magnet was balanced on a vertical steel pivot, in a box which occupied the centre of the horizontal circle. Attached to the magnet was a mirror, in which the reflected image of the cross wires was observed with a small telescope placed excentrically. The same telescope also served to observe the sun or a meridian mark for determining the geographical meridian.

The geographical positions were taken from Maps Nos. 740, 662, published by the Intelligence Division, War Office, or from astronomical observations taken on the spot; the former are indicated by * and the latter by †.

In each case the time of the observation is given as Cairo mean time, *i.e.*, $2^{h}\ 5^{m}\ 8^{s}\cdot9$ fast of Greenwich, since all the observations were made before the time of the 30° meridian E. of Greenwich was adopted as civil time for Egypt.

D'Abbadie's station, beside the Great Pyramid at Giza, was occupied on May 10, 1901, to obtain improved values of the secular variation. The observations were made between 1 P.M. and 4 P.M. when the electric tramway was not working.

Since there are, as yet, no self-registering magnetic instruments in

The times given are those of the middle of the observation, *i.e.*, for Girga—

		Mean.
Declination observation	8.58—9.11	9.5
First vibration	9.14—9.27	9.20
Second vibration...	9.34—9.47	9.40
Deflection	10.35—11.0	

The time of the horizontal force value is given as the middle of the time of the vibration observation.

Where one value is given for the dip one needle was used; where two values, the value obtained from each is given.

Deflection observations as well as the vibration observation were made on each occasion for the observations given in Table II(*b*); in the observations given on page 22 it is mentioned when it was not possible to take them.

I (*a*). Declination at Stations near Cairo determined with a Declinatorium.

Place.	Latitude N.	Longitude E.	Date.	Cairo mean time.	Declination West.	
	° ′ ″	° ′ ″		h. m.	°	
Mataria, site of ancient Temple of Heliopolis	*30 7 40	31 8 53	2–10–93	15 45	4 26·9	
Abbassia, The Polygon Camp....	*30 4 13	31 18 8	11–8–93	6 20	4 17·7	
” ” ”	” ”	” ”	11–10–93	16 10	4 24·6	
” ” ”	” ”	” ”	24–8–94	8 0	4 11·7	
” ” ”	” ”	” ”	26–10–94	7 0	4 17·0	
Cairo, Gezira Hut Barracks, S. end	*30 3 25	31 13 3	14–8–93	16 45	4 27·3	
Cairo, Venus Station on Jebel Moqattam	†30 1 46	31 16 39	17–8–93	18 20	4 35·9	Position determined by Transit of Venus Expedition, 1874.
Cairo, Moqattam Fort, south side	*30 1 31	31 16 6	20–9–93	16 58	4 24·6	
Mena House Hotel, Giza Pyramids	*29 59 30	31 7 46	1–9–93	15 15	4 30·4	
Saqqara, Mariette's House	*29 52 30	31 12 47	25–8–93	12 45	4 30·4	
Helwan, right bank of Nile......	*29 51 32	31 20 16	13–10–93	13 30	4 27·7	
Mit Rahini Village, left bank of Nile	*29 50 57	31 15 3	25–8–93	8 0	4 19·5	

I (*b*). Declination at Stations in the Lybian Desert near Kharga and Dakha Oases, determined with a Declinatorium.

Place.	Latitude N.	Longitude E.	Date.	Cairo mean time.	Declination West.	
	° ′ ″	° ′ ″		h. m.	° ′	
Assiut, left bank of Nile	27 11 0	31 12 0	15–12–93	9 40	4 47·5	Position given by Jordan, 1874.
On caravan road between Assiut and Kharga Oasis	26 50 0	30 58 15	18–12–93	13 30	4 52·7	Position fixed by compass traverse between Assiut and Kharga.
Kharga village	†25 26 20	30 32 50	22–12–93	10 0	5 1·5	
Beris, Kharga Oasis............	†24 42 33	30 36 0	27–12–93	17 0	4 54·8	
„ „ „	„ „	„ „	11–1–94	16 0	4 55·7	
On caravan road between Beris in Kharga Oasis and Mut in Dakhla Oasis	25 10 20	29 47 20	31–12–93	9 20	5 23·9	Position from compass traverse.
Mut, Dakhla Oasis	†25 29 4	29 4 40	2–1–94	17 0	5 22·8	
Point on caravan road south of Mut	24 16 45	29 14 30	7–1–94	14 30	5 32·0	Position from compass traverse from Mut to Beris.
Point in Desert west of Beris....	24 37 30	30 20 40	9–1–94	15 10	4 56·2	„ „
Point on Derb el Arbain caravan road south of Beris	23 52 30	30 27 40	13–1–94	15 30	5 14·2	„ „
Bir Murr	†23 19 30	30 11 40	15–1–94	9 5	5 3·2	

I (*c*). Declination at Stations in Wadi Natrun and Baharia Oasis, determined with a Declinatorium.

Wadi Natrun, Deir Anba Bishoi	†30 19 25	30 25 0	16–4·94	10 30	4 38·3	
„ „ Deir Baramus....	†30 21 18	30 16 24	17–4·94	16 0	4 54·0	
Baharia Oasis, Mandisha........	†28 21 00	28 58 0	24–4–94	8 30	5 8·9	

etermined with a Declinatorium.

	Cairo mean time.	Declination West.	
	h. m.	°	
3	15 45	4 26·9	
3	6 20	4 17·7	
3	16 10	4 24·6	
4	8 0	4 11·7	
4	7 0	4 17·0	
3	16 45	4 27·3	
3	18 20	4 35·9	Position determined by Transit of Venus Expedition, 1874.
3	16 58	4 24·6	
3	15 15	4 30·4	
3	12 45	4 30·4	
3	13 30	4 27·7	
3	8 0	4 19·5	

I (*b*). *Declination at Stations in the Lybian Desert near Kharga and Dakha Oases, determined with a Declinatorium.*

Place.	Latitude N.	Longitude E.	Date.	Cairo mean time.	Declination West.	
	° ′ ″	° ′ ″		h. m.	° ′	
Assiut, left bank of Nile	27 11 0	31 12 0	15–12–93	9 40	4 47·5	Position given by Jordan, 1874.
On caravan road between Assiut and Kharga Oasis	26 50 0	30 58 15	18–12–93	13 30	4 52·7	Position fixed by compass traverse between Assiut and Kharga.
Kharga village	†25 26 20	30 32 50	22–12–93	10 0	5 1·5	
Beris, Kharga Oasis............	†24 42 33	30 36 0	27–12–93	17 0	4 54·8	
„ „ „	„ „	„ „	11–1–94	16 0	4 55·7	
On caravan road between Beris in Kharga Oasis and Mut in Dakhla Oasis	25 10 20	29 47 20	31–12–93	9 20	5 23·9	Position from compass traverse.
Mut, Dakhla Oasis	†25 29 4	29 4 40	2–1–94	17 0	5 22·8	
Point on caravan road south of Mut	24 16 45	29 14 30	7–1–94	14 30	5 32·0	Position from compass traverse from Mut to Beris.
Point in Desert west of Beris....	24 37 30	30 20 40	9–1–94	15 10	4 56·2	„ „
Point on Derb el Arbain caravan road south of Beris	23 52 30	30 27 40	13–1–94	15 30	5 14·2	„ „
Bir Murr	†23 19 30	30 11 40	15–1–94	9 5	5 3·2	

I (*c*). Declination at Stations in Wadi Natrun and Baharia Oasis, determined with a Declinatorium.

Wadi Natrun, Deir Anba Bishoi	†30 19 25	30 25 0	16–4–94	10 30	4 38·3	
„ „ Deir Baramus....	†30 21 18	30 16 24	17–4–94	16 0	4 54·0	
Baharia Oasis, Mandisha........	†28 21 00	28 58 0	24–4–94	8 30	5 8·9	

Description of Stations observed at with Declinatorium.

Place.	Description.
Mataria	On ruins of Temenos wall of Temple of Heliopolis; S. of obelisk.
Abbassia	N. end of Polygon, E. of Suez road, E. end of row of trees behind battery.
Gezira............	100 metres north of Grotto, N.W. of Gezira.
Venus Station, Jebel Moqattam.	Observation point of Transit of Venus Expedition, 1874.
Moqattam Fort	200 metres south of old fort on spur above Citadel, *not* the earthwork on the top of the hill by the Cholera

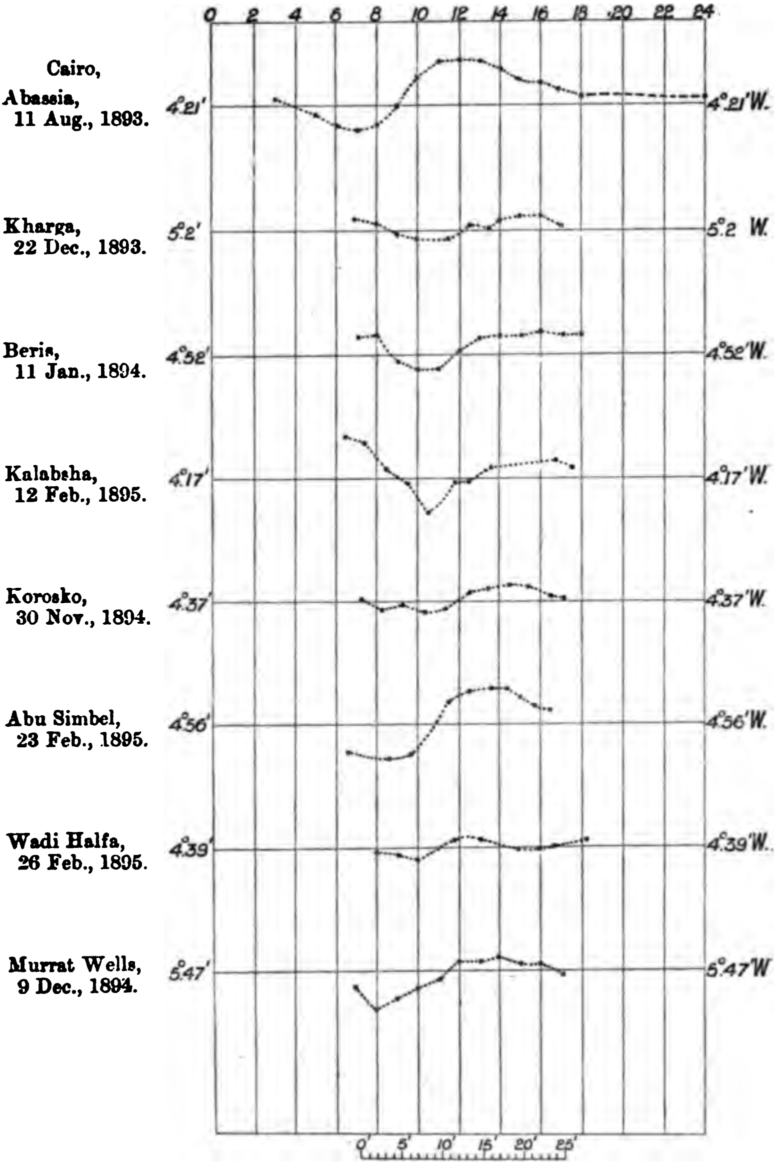
II (*a*). Declination at Stations in the Nile Valley, determined with Kew pattern Magnetometer No. 73.

Place.	Latitude N.	Longitude E.	Date.	Cairo mean time.	Declination West.	
	° ′ ″	° ′ ″		h. m.	° ′	
Abbassia, Polygon Camp........	*30 4 13	31 18 8	27–9–95	8 20	4 3·9	
" " "	" "	" "	11–6–96	9 30	4 5·4	
Girga, left bank of Nile	*26 20 10	31 53 25	24–11–94	9 5	4 29·3	
Dendera, left bank of Nile	*26 10 0	32 45 0	3–11–95	11 40	4 43·2	
Luxor, right bank of Nile	25 41 50	32 38 34	15–3–95	15 10	4 26·8	Position from Venus Transit Expedition, 1874.
Esna, left bank of Nile	25 18 0	32 34 0	13–3–95	12 55	4 51·8	Position from Jordan, 1874.
Edfu " "	24 58 40	32 53 5	30–12–94	8 0	4 7·3	" " "
Assuan, Elephantine Island	†24 5 0	32 52 0	6–2–95	9 0	4 22·6	
1st Cataract, South end, Bab el Gedid	*24 2 30	32 51 0	9–3–95	11 0	4 33·1	
Philæ, Great Temple	†24 1 10	32 51 50	20–12–95	14 50	4 24·6	
" " "	" "	" "	11–4–96	10 7	4 22·9	
Dabod, left bank of Nile	†23 53 40	32 53 10	9–2–95	16 30	4 21·7	
Taifa " "	†23 37 30	32 53 20	7–3–95	13 40	4 23·3	
Kalabsha " "	†23 32 10	32 53 25	12–2–95	6 30	4 22·1	
Abu Hor, right bank of Nile	†23 28 15	32 55 0	13–2–95	15 20	4 29·0	
Gerf Hussein, left bank of Nile..	†23 17 20	32 56 25	15–2–95	9 30	4 27·2	
Dakka " " ..	†23 9 5	32 47 15	16–2–95	8 40	4 18·7	
Uffeduni " " ..	†23 0 54	32 42 12	17–2–95	8 30	4 27·6	
Sebua " " ..	†22 45 5	32 35 10	20–2–95	8 40	4 35·0	
Korosko, right bank of Nile	†22 56 38	32 18 5	30–11–94	9 20	4 35·9	
Amada, left bank of Nile........	†22 43 30	32 16 30	5–3–95	14 0	4 37·9	
Abu Simbel, left bank of Nile....	†22 19 40	31 39 0	23–2–95	14 40	5 0·2	
Akshi (Serra) " "	†22 8 0	31 20 9	2–3–95	10 10	5 25·4	
Wadi Halfa, right bank of Nile..	†21 54 49	31 19 2	26–2–95	11 40	4 39·9	
Amka (ancient fort Mirgissi) left bank of Nile	*21 48 30	31 11 0	28–2–95	12 20	4 55·7	
Murrat Wells, Nubian Desert....	†21 3 54	32 54 40	9–12–95	8 10	5 42·3	
Wadi Mogharin " "	†21 20 35	32 20 0	14–12–95	17 20	3 48·6	

ce in the Nile Valley.

Cairo mean time.	Horizontal force.		No. of dip needle.
h. m.			
10 20	0·30076	—	2
16 0	0·30038	Lat. 28° 38′ 50″	2
—	—	—	—
—	—	—	1
9 20	0·31608	—	1
9 40	0·31622	—	2
—	—	—	1 2
11 15	0·31874	—	—
11 45	0·31904		
15 30	0·32070	—	2
—	—	Lat. 25° 29′ 0″	1
—	—	—	1
8 30	0·32357	—	—
7 15	0·32364	Lat. 24° 7′ 0″	1
—	—	—	1 2
18 15	0·32440	—	1 2
16 40	0·32390	—	—
—	—	—	1 2
13 30	0·32716	—	1 2
7 0	0·32926	—	1 2
15 40	0·32702	—	1
12 20	0·32815	—	1 2

Curves of Diurnal Variation of Declination.



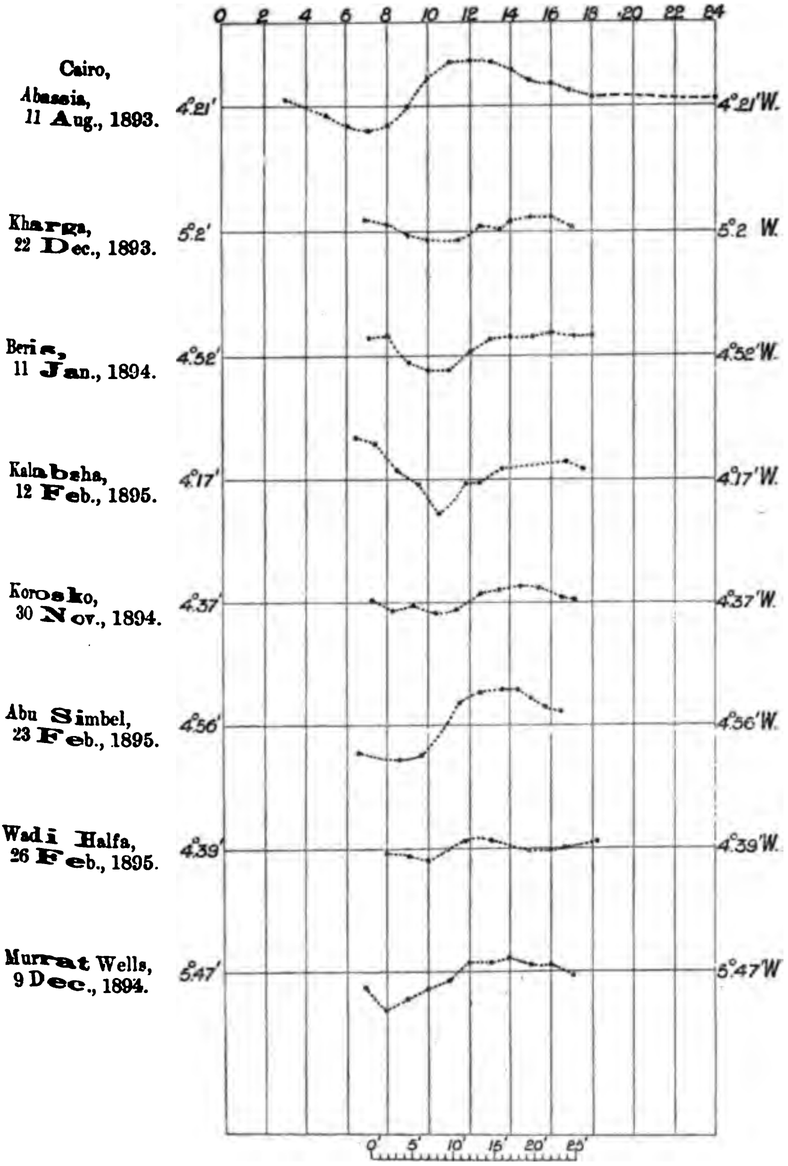
Scale of twenty-five minutes of arc.

Abbassia, Kharga, and Beris observed with Declinatorium, the rest with Kew pattern magnetometer.

Description of Stations observed at with Magnetometer and Dip Circle.

Place.	Description.
Abbassia	The same as for Declinatorium.
Girga	S.W. of huts, which are 150 metres west of canal head on N. side of steamer landing barge.
Dendera	100 metres E. of point where steamers stop.
Luxor	On river bank, 50 metres north of house painted red and white, on bank south of Luxor.
Mualla	2 kiloms. S. of irrigation resthouse.
Esna	100 metres north of landing barge, and on river bank.
Edfu	40 metres from bank, and 120 metres S. of landing barge.
Assuan*	Rest camp, S. side of 2nd hut from N.W. corner.
Elephantine*	25 metres S.E. from gateway of ancient temple.
1st Cataract*	South side of the Bab el Gedid.
Philæ*	Till 29 Nov. E. side of kiosk, on quay, after then on roof of Temple of Isis, N. end.
Dabod	200 metres N. of ancient masonry quay.
Taifa*	100 metres from S.E. corner of village, on river bank.
Kalabsha*	30 metres from river bank, and 70 metres north of great temple.
Abu Hor*	On river bank, 100 metres S. of steamers' stopping place.
Gerf Hussein	At entrance to temple.
Dakka	S.E. angle of temple.
Uffeduni	On eastern part of temple ruins

Curves of Diurnal Variation of Declination.



Scale of twenty-five minutes of arc.

Abbassia, Kharga, and Beris observed with Declinatorium, the rest with Kew pattern magnetometer.

W. 4° +

Hours.	1.	2.	3.	4.	5.	6.	7.	8.
7	26 ·8	28 ·2	—	27 ·1	25 ·5	26 ·4	25 ·7	No observations.
8	25 ·3	28 ·0	27 ·8	26 ·9	25 ·7	—	—	
9	24 ·6	26 ·2	26 ·8	—	—	26 ·6	—	
10	26 ·6	27 ·7	26 ·8	28 ·6	28 ·4	28 ·0	28 ·4	
11	26 ·9	28 ·6	—	28 ·4	30 ·0	29 ·6	30 ·0	
Noon	27 ·1	28 ·0	29 ·8	27 ·8	29 ·5	—	28 ·4	
1	—	27 ·7	28 ·2	—	26 ·4	28 ·4	—	
2	27 ·1	—	28 ·2	26 ·0	—	27 ·3	—	
3	28 ·0	27 ·5	27 ·1	26 ·2	—	—	26 ·4	
4	27 ·8	—	28 ·0	24 ·6	26 ·0	26 ·6	—	
5	27 ·5	—	—	27 ·8	—	26 ·2	—	

W. 4° +

Hours.	1.	2.	3.	4.	5.	6.
7	—	—		26 ·2	—	—
8	—	27 ·7		25 ·7	24 ·6	25 ·7

Diurnal Variation of Declination at Philæ, Assuan.

. 24° 1′ 10″ ; Long. E. 32° 51′ 50″, from December, 1895, to April, 1896.

employed on Philæ Island examining and restoring the uildings on the island, magnetic observations in the neigh- country could not be undertaken for want of time. A neter was set up, therefore, in a small, empty chamber on the the Great Isis Temple, and the declination observed hourly .M. to 5 P.M., except when the work on the excavations pre- The results are given in the following tables (see also p. 12):—

December, 1895.

0.	21.	22.	23.	24.	25.	26.	27.	28.	29.	30.	31.
—	26·9	26·0	26·4	26·4	25·7	—	26·0	No observations.	25·5	26·8	26·2
·6	—	—	—	25·9	25·5	—	24·4		24·4	24·8	—
·1	25·1	25·3	25·9	25·9	26·8	24·8	23·7		24·8	27·8	25·0
·6	24·8	24·6	27·6	—	—	27·1	25·0		26·6	—	—
·1	25·0	—	29·6	28·0	29·3	27·8	25·2		27·7	27·8	28·4
·1	—	26·0	29·4	28·2	28·8	27·6	24·1		27·5	—	28·6
—	24·6	—	—	—	—	—	23·7		—	—	—
·6	25·1	24·8	27·7	26·0	26·4	26·2	—		26·2	—	27·7
·6	—	—	25·8	24·2	26·0	25·7	23·7		26·6	27·7	28·1
·9	24·6	—	25·5	—	—	25·7	24·6		26·8	—	28·1
·7	25·8	24·6	26·2	24·4	—	—	—		26·9	26·8	27·9

lding tables for months of January, February and March.)

W. 4° + April, 1896.

Hours.	1.	2.	3.	4.	5.	6.	7.	8.	9.
7	—	22·8	—	—	—	—	—	—	—
8	22·3	—	22·8	21·5	21·0	19·9	20·5	20·8	19·2
9	—	24·4	—	22·3*	—	—	—	—	—
10	—	26·6	—	—	—	—	21·5	—	20·9
11	24·6	28·0	27·1	—	23·7	23·5	24·6	—	22·8
Noon	25·5	—	—	25·5	24·9	24·8	26·0	24·6	24·6
1	—	—	—	—	—	—	—	—	—
2	24·6	28·2	26·0	24·6	24·6	24·2	25·8	26·0	22·9
3	—	—	—	—	—	—	—	—	—
4	—	24·6	—	—	—	—	—	—	—
5	22·9	—	22·8	22·1	22·3	21·0	22·8	21·9	22·6

IV. *Magnetic Observations at Helwan, near Cairo.*

(See table on opposite page.)

V. *Secular Variation.*

As many early observations as it has been possible to find in various

IV. Magnetic Observations at Helwan, near Cairo, 1898.

Date.	Time.	Dip.	Time.	Horizontal force.	Time.	Declination.	Notes.
	h. m.	° ′	h. m.		h. m.	° ′ ″	
October 27	15 42	40 31·6(1)*	—	—	—	—	(1) Dip needle 1.
November 1	14 50	0 31·2(1)*	—	—	—	—	(2) „ „ 3.
December 9	13 37	0 32·0(1)	—	—	12 35	4 15 45	(3) Uncorrected for torsion. This correction was almost invariably positive.
„ 9	14 17	0 27·8(2)	—	—	—	—	
„ 14	15 41	0 27·9(2)	14 21	0·30182	13 53	4 15 13	
„ 24	15 33	0 28·1(2)	14 10	0·30211	13 54	4 14 46	
„ 29	14 37	0 30·8(2)*	—	—	16 52	4 13 23(3)	
Mean values for 1899	—	40 32·1	—	0·30132	—	4 11 17	

The details of the above observations are given in the Meteorological Report of the Survey Department, Cairo, for 1898 and 1899.

* Observed by Mr. J. J. Craig.

V. Secular Variation of Declination.

Place.	Observer.	Date.	Declination west.			Change per annum.	
			°	′	″	′	
Alexandria	Quesnot and Nouët.	1798	13	6	0	—	
,, ...	?	1842	8	24	0	−6·4	
,, ...	Güssfeldt.	1876	5	55	9	−4·4	
,, ...	Capt. Leslie, R.E.	1890	5	3	0	−3·8	
,, (Ramleh)	d'Abbadie.	1884	5	6	2	—	
Cairo	Russegger.	1839	9	2	0	—	
,, (Old Cairo) ..	d'Abbadie.	1885	5 5	15 28	9 3	−4·8	
,, Abbassia	H.G.L.	1895	4	5	0	−7·7	'39-95 =
,, J. Moqattam..	d'Abbadie.	1885	4	56	5	—	
	H.G.L.	1893	4	36	0	−2·5	
,, Great Pyramid	d'Abbadie.	1885	4	48	9	—	
,, ,, ,,	H.G.L.	1893	4	32	0	−2·1	
,, ,, ,,	H.G.L.	1901	3	48	6	−5·5	'85-01 =
Helwan	H.G.L.	1893	4	27	7	—	
,, ...	Mean for 1899.	1899	4	11	3	−2·7	

V. Secular Variation of Declination—*continued.*

Place.	Observer.	Date.	Declination west.			Change per annum.	
			°	′	″		
Assuan	Cailliaud.	1819	12	0	0	—	
	d'Abbadie.	1885	5	13	2	−6·1	
	H.G.L.	1895	4	26	0	−4·7	
Amada............	Cailliaud.	1819	11	13	0	—	At Tomas, 6 kiloms. up-stream
	H.G.L.	1895	4	38	0	−5·2	
Wadi Halfa.......	Cailliaud.	1819	11	30	0	—	
	H.G.L.	1895	4	40	0	−5·4	
Mirgissi (Amka) ..	Cailliaud.	1819	13	0	0	—	At Sarras, 20 kiloms. south
	H.G.L.	1895	4	56	0	−6·2	

The magnetic bearing of the walls or axes of certain temples is given in the "Description de l'Égypte," and from these an approximate value for the secular change in the declination may be deduced.

On Philæ—

	Magnetic bearing, 1799.	True bearing.	Declination, 1799.
Great Isis Temple......	44° E. of N.	32° 5′ E. of N.	11° 55′
Nectanebo's Temple....	25° „	13° 28′ „	11° 32′
The Kiosk (or Pharaoh's Bed)	104° 30′ „	93° 25′ „	11° 5′
		Mean........	11° 31′

The Declination in 1896 was 4° 23′ W., which gives an average annual decrease of 4′·4.

Kom Ombo Temple—

Magnetic bearing of axis, 1799......	55°	E. of N.
„ „ 1892......	43° 55′	„
Change in 93 years	11° 5′	
„ per annum	7′·2	

Edfu Temple—

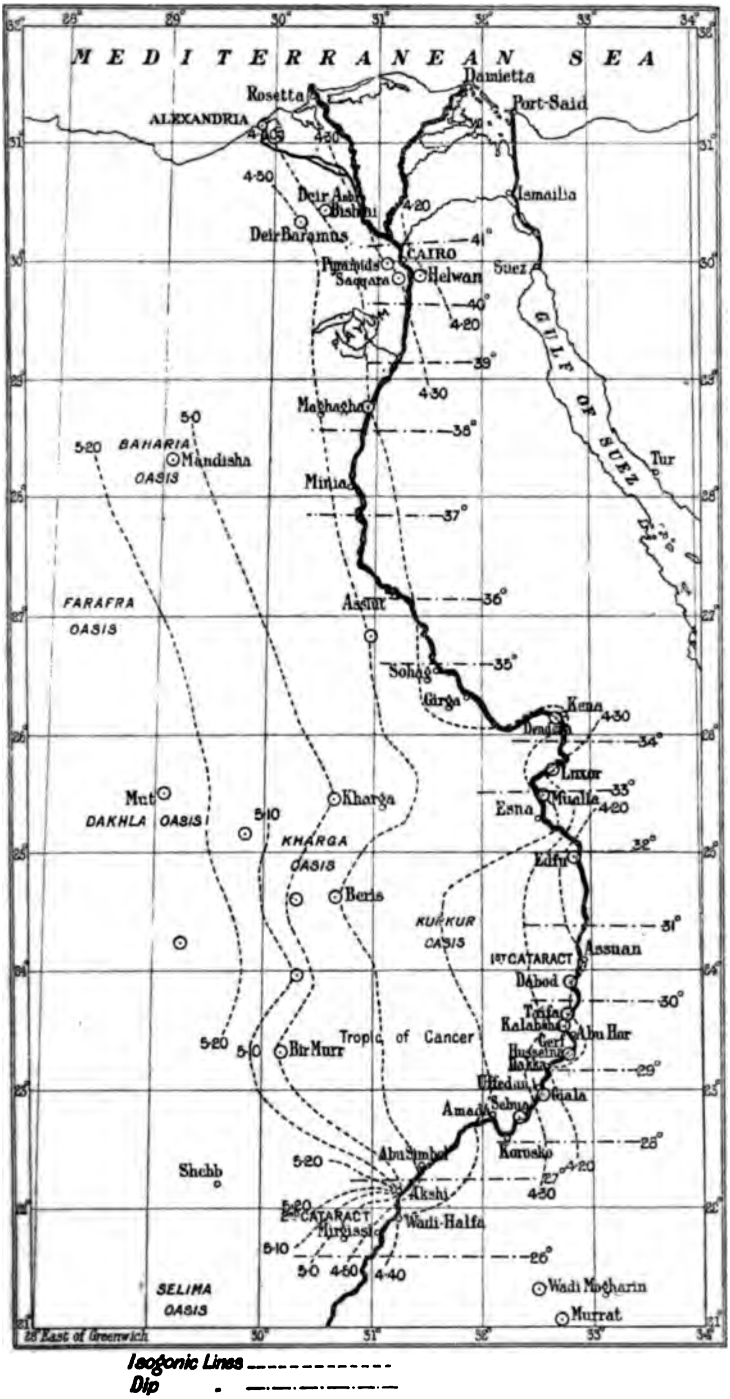
Magnetic bearing of axis, 1799	15°	E. of N.
„ „ 1892	3° 10′	„
Change in 93 years	11° 50′	
„ per annum	7′·7	

Secular Variation of Dip.

Place.	Observer.	Date.	Dip.	Yearly change.
			° '	'
Alexandria	Quesnot and Nouet.	1798	47 30	—
"	?	1842	43 48	−5·0
"	Güssfeldt.	1876	42 52	−1·6
" (Ramleh)	d'Abbadie.	1884	42 47·7	−0·7
Cairo—				
Great Pyramid....	d'Abbadie.	1839	41 41·8	—
" "	"	1885	40 46·8	−1·2
	H.G.L.	1901	40 31·6	−1·0
Luxor	d'Abbadie.	1885	33 7·7	
	H.G.L.	1895	33 38·1	+3·0
Assuan	—	—	—	—

Secular Variation of Horizontal Force.

			H. in	


MEDITERRANEAN SEA
Rosetta
Damietta
Port-Said
ALEXANDRIA
4·30
4·50
Deir Abu Bishai
4·20
Ismailia
Deir Baramus
41°
CAIRO
Pyramids
Saqqara
Helwan
Suez
40°
4·20
FAYUM
39°
GULF OF SUEZ
4·30
Maghagha
5·0
38°
5·20
BAHARIA OASIS
Mandisha
Tur
Minia
37°
FARAFRA OASIS
Assiut
36°
Sohag
35°
Girga
Kena
4·30
Dendera
34°
Luxor
Mut
Mualla
33°
Esna
4·20
DAKHLA OASIS
Kharga
5·10
KHARGA OASIS
32°
Edfu
Beris
KURKUR OASIS
31°
1st CATARACT
Assuan
Dabod
30°
Tafa
Kalabsha
Abu Hor
Gerf Hussein
Dakka
29°
Tropic of Cancer
5·20
5·0
Bir Murr
Wady Aladi
Gala
Amada
Sebua
28°
Korosko
4·20
Shebb
5·20
Abu Simbel
27
4·30
Akshe
5·20
2nd CATARACT
Wadi Halfa
Mirgissa
5·10
26°
5·0
4·50
4·40
SELIMA OASIS
Wadi Mogharin
Murrat
28° East of Greenwich
29°
30°
31°
32°
33°
34°
Isogonic Lines
Dip

These appear to show abnormally high values at Qena where the faulting of the Nile Valley is highly developed, and also at Esna and at Akshi (Serra) between Abu Simbel and Wadi Halfa. At this place, too, the horizontal force has a high value.

APPENDIX.

Observations on the Upper Nile.

The following observations were taken in March and April of the present year (1901) while accompanying Sir W. E. Garstin, K.C.M.G., Under-Secretary of State for Public Works, from Khartum through the region of the "Sadd" to Gondokoro and back, and are added here with his permission. As this journey was specially undertaken to see the upper reaches of the Bahr el Jebel, and to measure the discharge of the White Nile and its various tributaries in this district, magnetic observations had to be taken whenever opportunities occurred. It has consequently happened that the stations occupied cannot be described with sufficient accuracy for them to be re-occupied at a future date, since most of them were wooding-stations with no permanent building or other marks in the neighbourhood.

The latitudes given are taken from the map of the Bahr el Abiad (White

Place.	Lat. N.	Long. E.	Declination. 1863-64.
J. Abel	12° 42′	34° 25′	9° 10′
J. Qerebin	12 8	34 15	20 01
Werkat (southern part)	12 8	34 10	6 30
Roro	11 54	34 3	12 05
Gule	11 43	33 57	8 40

Also Captain A. W. Peel gives the West Declination at Khartum, as 8° 30′ in October, 1851, and Lieut. Watson, R.E., gives 7° 30′ West, for that of Rejaf (lat. 4° 40′ ±), on 15th December, 1874.

The only other observation it has been possible to find in the books of travel, &c., available in Cairo, is a value of 7° 30′ W. for the declination at Gondokoro on March 20, 1861,* which with 6° 20′ for the present values gives − 2′ per annum.

Unfortunately the first rains were already threatening at Lado and Gondokoro, and the sky was usually too cloudy to admit of satisfactory observations for azimuth, Hellet el Nuer and El Kenisa were therefore the only two places where the declination could be determined on the Bahr el Jebel. Russegger also gives for El Obeid declination 8° 30′ west and dip 18° for April, 1837, but neither of these can be utilised, being too far from the Nile.

The instruments used were Kew-pattern Magnetometer, No. 87, by Elliott, and a Dip Circle, No. 131, by Dover.

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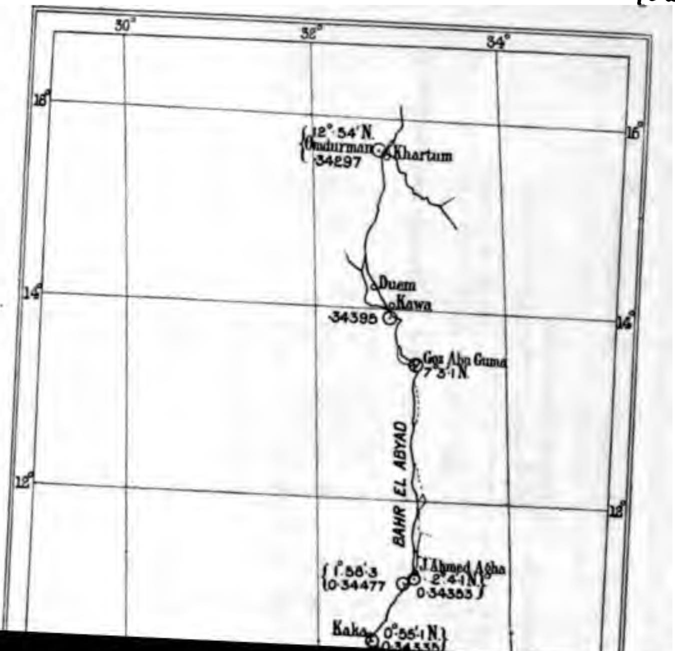
* Peney, 'Bull. Soc. Géog. Paris,' 1863.

Bahr el Abyad and the Bahr el Jebel.

[redacted]	Mean time. 30° E.	Horizontal force.	
[redacted]	—	0·34297	
[redacted]	—	0·34395	
[redacted]	—	—	
[redacted]	—	0·34353	
[redacted]	—	0·344774	
[redacted]	—	0·34335	
[redacted]	—	0·34452	
[redacted]	—	0·34198	
[redacted]	—	0·34299	1° 34′·9 obtained from obs. 10° E. and W. of meridian.
[redacted]	—	—	
[redacted]	—	0·33820	No deflection obs.
[redacted]	—	0·33529	
[redacted]	—	0·33506	
[redacted]	—	0·33387	No deflection obs.
[redacted]	—	0·33164	

Magnetic Declination determined on White Nile and Bahr el Jebel with Kew-pattern Magnetometer No. 87.

Place.	Date.	Latitude, N.			Mean time 30° E.		Declination west.		
		°	′	″	h.	m.	°	′	
Omdurman, near steamer workshops	12·4–1901	15	39	40	17	28	4	39·5	
Renk	13·3-1901	12	0	0	8	0	6	4·5	Probably local disturbance.
East bank, 6 kiloms. south of Jebel Ahmed Agha	8–4–1901	11	1	0	6	45	5	7·1	
West bank, south of Jebel Ahmed Agha	14-3-1901	10	58	0	9	20	4	57·6	
Wood station, near Kaka, right bank	17–4–1901	10	30	0	14	5	4	55·7	
Wood station, right bank, 48 kiloms. north of Fashoda	15–3–1901	10	20	0	8	30	5	8·9	
Wood station, right bank, 8 kiloms. south of mouth of R. Sobat	16–3–1901	9	22	0	16	10	5	43·7	
Left bank of Bahr el Jebel, 10 kiloms. south of Hellet Nuer	20–3–1901	8	9	30	17	15	5	49·6	
Kenisa, left bank of Bahr el Jebel	23–3–1901	6	46	0	16	20	6	13 8	


30°
32°
34°
16°
14°
12°
12°·54′ N.
Omdurman
·34297
Khartum
Duem
Kawa
·34395
Gez Abu Guma
7°3′·1 N.
BAHR EL ABYAD
1°.58′·3
0·34477
J. Ahmed Agha
2°·4·1 N.
0·34353
Kaka
0°·55′·1 N.

Description of Stations.

Place.	Bank of river.	Observations made.
Omdurman, 200 metres south of steamer workshops	Left	Declination. Dip. Horizontal force.
Kawa (lat. 13° 55′)...............	,,	Horizontal force.
Goz Abu Goma (lat. 13° 23′)......	Right	Dip.
Renk.........................	,,	Declination.
6 kilometres up stream of Jebel Ahmed Agha	,,	Declination. Dip. Horizontal force.
16 ,, ,, ,, ,,	Left	,, ,, ,,
Near Kaka (lat. 10° 30′)..........	Right	,, ,, ,,
48 kilometres down stream of Fashoda (lat. 10° 20′)	,,	,, ,, ,,
8 kilometres up stream of mouth of River Sobat, on Bahr el Abyad	,,	Declination. Horizontal force.
Wood station on Bahr el Abyad, east of mouth of Bahr el Zaraf 12 kilometres	,,	Dip. Horizontal force.
Bahr el Zaraf, 20 kilometres from mouth	Left	Dip.
Bahr el Jebel, 10 kilometres....... South of Hellet Nuer (lat. 8° 9′ 30″)	,,	Declination. Horizontal force.
Kenisa (lat. 6° 46′ 0″)..........	,,	Declination. Dip. Horizontal force.
Wood station, south of Bor on Bahr el Jebel (lat. 6° 5′)	Right	Dip. Horizontal force.
Wood station, south of Kiro on Bahr el Jebel (lat. 5° 15′)	,,	,, ,,

"Note on the Effect of Mercury Vapour on the Spectrum of Helium." By Professor J. NORMAN COLLIE, F.R.S. Received June 3,—Read June 19, 1902.

Some years ago the author, in conjunction with Professor Ramsay, published the results of some experiments relating to the visibility of the spectrum of one gas in presence of another.*

Since then some experiments have been made on the effect of mercury vapour (when present in considerable quantity) on the spectrum of helium in an ordinary Plücker's tube, under the influence of the electric discharge from an induction coil. When the spectrum of helium is examined in an ordinary Plücker's tube, it appears to be a simple one consisting of only eight lines—two red, one yellow, three green, one blue, and one violet.

The spectrum, however, of the negative glow is much more complicated, a large number of other and fainter lines appearing as well. If mercury vapour be introduced into such a tube filled with helium at from 2—5 mm. pressure, the first change noticed is the simplification of the spectrum of this negative glow; and at the same time three of the helium lines disappear from the spectrum of the negative glow, the red 7065, the blue 4713, and the violet 4472, whilst the yellow 5876 becomes very feeble. In the orange part of the spectrum a brilliant new line can now be seen, namely, the mercury line 6151, a line which as a rule does not appear in a Plücker's tube containing mercury vapour when an electric discharge from an induction coil is passed through it. The presence of the orange line of mercury is of interest, as other gases, argon, krypton, hydrogen, &c., mixed with mercury vapour, when examined under the same conditions fail to give it.

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* 'Roy. Soc. Proc.,' vol. 59, p. 262.

Whether this orange line is shown in a neon tube where mercury vapour is present is difficult to say, as one of the bright orange lines of neon almost coincides with it.*

Besides this alteration of the helium spectrum at the negative electrode another curious change can be observed, if a piece of tubing (whose internal diameter is about 4 mm.) be introduced into the centre portion of the Plücker's tube. The spectrum of the gas in this glow coincide with these two systems. For all those lines which disappear in the negative glow, 7065, 4713, 4472, and the yellow line 5876 which nearly does so, belong to that system which, according to Runge and Paschen, is due to the gas helium; whilst these which can be seen at the negative glow with their full brilliance, 6677, 5016, and 4922, belong to the second system.

This differentiation of the helium spectrum brought about by the presence of mercury vapour might at first sight appear as a confirmation of the idea of Runge and Paschen that helium is a mixed gas, consisting of two different elements. But taken in conjunction with the fact that the spectra of argon, neon, and krypton are all altered by the same treatment, no reliance necessarily can be placed on the argument.

In conclusion it is worth while pointing out that a helium-mercury tube containing the merest trace of hydrogen should be of value as a standard tube for spectroscopic measurements. For it contains fourteen standard lines, amongst them (C and F). Moreover all these lines are of brilliance ten, very equally spaced from the extreme red to the violet, and with dark spaces between them.

He	Red	7065	Hg	Green	5461
He	Red	6677	He	Green	5016
H	Red	6563 C	He	Green	4922
Hg	Orange	6151	H	Green	4861 F
He	Yellow	5876	He	Blue	4713
Hg	Yellow	5790	He	Violet	4472
Hg	Yellow	5769	Hg	Violet	4359

"The Seed-fungus of *Lolium temulentum*, L., the Darnel."

By E. M. FREEMAN, M.S., University of Minnesota. Communicated by Professor MARSHALL WARD, F.R.S. Received June 6—Read June 19, 1902.

(Abstract.)

Darnel (*Lolium temulentum*, L.) has been known since Roman times for the poisonous properties of its grain. It was not, however, until 1898 that the presence of an often considerable layer of hyphæ was discovered just exterior to the aleurone layer of the grain; to the action of this fungus-layer the poisonous properties are presumably due.* Nothing had hitherto been known regarding the method of nfection of the plant, although the presence of hyphæ in the growing oint from about the 8th day of germination to the formation of the ruit had been observed by one investigator, and but little positive vidence as to the real nature of the fungus has been produced.

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* How far ergot and other fungi may be concerned is a disputed point.—[June 24.]

In addition to the now known hyphal layer outside the aleurone of he grain the author has discovered a hitherto unnoticed patch of yphæ, just outside of and contiguous to the base of the scutellum. rom this patch hyphæ can be found penetrating to the growing point f the embryo in the seed, and an abundant mycelium can usually be etected in the young growing point. The hyphæ continue their rowth, always intercellular, in the growing point and in the leaf-bases ntil the formation of the inflorescence, when every young ovule conains an abundance of hyphæ throughout the nucellus, and extending to ithin two cells of its exterior. When the ovule elongates and assumes he ovoid form, there is formed a tongue of hyphæ extending in the nuellus, from the funicular side to the micropyle on the inner or axial side f the embryo-sac. This patch becomes isolated by the further elongaion of the ovule, and by the cessation of growth of the hyphæ in the unicle, and remains vigorous, forming an *infection layer*. When the mbryo has attained a length of about 3/10 mm. and the rudiments f a lateral growing point and a terminal scutellum have been formed,

Summary.

1. The proportion of grains without to those with the fungus layer outside of the aleurone of *L. temulentum* varies, but is usually from 80—100 per cent. In some cases the microscopic differences are sufficient for diagnostic purposes.

2. The hyphæ sometimes penetrate the aleurone at any point and invade the starch-endosperm, but no fructification or spores of any kind have been seen.

3. There exists in the nucellus, at the base of the scutellum and at the lower end of the inner groove of the grain, a layer of hyphæ which lies directly against the embryo, constituting an *infection layer*.

4. Embryos of the grain of *L. temulentum* in a proportion approximately equal to that of the occurrence of the fungus in the grains, always contain hyphæ in the growing point, and these hyphæ can be traced to their origin in the *infection layer*. They remain in the growing point throughout the life of the plant.

5. The course of the hyphæ is always intercellular.

6. There is no reason for the supposition of any so-called "mycoplasm" in the embryo: the fungus always exists as distinct hyphæ.

7. In the growing plant the fungus forms networks in the leaf-bases, of which the function has not yet been determined.

8. The nucellar hyphæ of the grain undergo degeneration during germination without the formation of spores.

9. All attempts to obtain cultures of the nucellar hyphæ have failed, indicating either that the hyphæ here have lost their vitality or else, as is more probable, that they are too closely adapted to a parasitic (or symbiotic) life to allow of artificial culture.

10. In the young ovary a tongue of hyphæ reaching from the funicle to the micropyle on the inner side of the grain, becomes detached from the remaining nucellar hyphæ by the elongation of the ovule and the cessation of hyphal growth in the funicle.

11. Infection takes place from this layer as soon as the growing point of the embryo has appeared as a recognisable rudiment.

12. The fungus of other species seems very probably identical with that of *L. temulentum*, as cross-infections seem to be possible by grafting. *L. multiflorum*, Lam., and *L. italicum*, Braun., also contain a hyphal layer.

13. There are serious objections to the reference of this fungus to the Ustilagineæ, to the ergot-forming Pyrenomycetes or the Hyphomycetes (such as Woronin and Prillieux and Delacroix have described for poisonous rye), or to the suggestion that it is a Uredine.

14. The effect of the fungus upon its host seems rather beneficial than detrimental, and several facts point toward the confirmation of

Guerin's suggestion of a symbiotic relationship. Such can be conceived of as arising from a former sclerotium- (or spore-) forming habit by the adoption of a new intra-seminal mode of infection.

"On the Effects of Magnetisation on the Electric Conductivity of Iron and Nickel."

By GUY BARLOW, B.Sc., Research Fellow of the University of Wales. Communicated by Professor A. GRAY, F.R.S.

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That magnetisation has an effect on the electric conductivity of metals was first noticed in 1856 by William Thomson[*] (Lord Kelvin), and since then, on account of its very great theoretical interest, this phenomenon has formed the subject of numerous experiments. These later investigations have been made by Tomlinson,[†] Righi,[‡] Hurion,[§] Leduc,[||] Ettingshausen and Nernst,[¶] Ettingshausen,[**] Goldhammer,[††] Lenard and Howard,[‡‡] Lenard,[§§] Henderson,[||||] Beattie,[¶¶] Gray and Jones,[***] and others.

Two cases of the phenomenon must be carefully distinguished—

ance divided by the resistance in zero field; and a is a constant. It is therefore suggested by Goldhammer that the above relation may be also true for the ferro-magnetic metals. In order to verify this a direct determination of the magnetisation is necessary. Beattie has investigated the transverse effect in thin films of iron, nickel, and cobalt, and determined the magnetisation by means of Kundt's result that for such films the Hall effect and the magnetisation are proportional. The relation $\Delta\phi = a\mathrm{I}^2$ was confirmed in the case of the cobalt films, but not in those of nickel. In iron the variation of resistance was so small that accurate results were not obtained. In the more recent investigation of Gray and Jones on the change of resistance of iron wire magnetised longitudinally, the relation $\Delta\phi = a\mathrm{I}^4$ was suggested as approximately representing their results between the field strengths 30 and 250 c.g.s.

The present investigation had for its object a more exact determination of the relation between the change of resistance and the magnetisation in the two ferro-magnetic metals—iron and nickel. As in the experiments of Gray and Jones the metal was in the form of a wire, so that a close approximation to uniform longitudinal magnetisation could be obtained. In the course of the investigation the hysteresis of resistance was found to be of such importance that this phenomenon was separately examined. Some determinations of the transverse effect were also included.

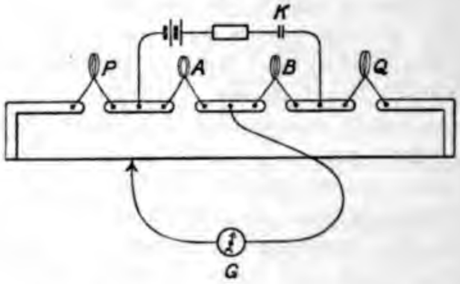
Experiments in Ordinary Magnetic Fields.

These experiments were made with three different specimens of wire, iron, steel, and nickel.

The magnetic field was obtained by means of a solenoid 1 metre in length, which was placed vertically. The current for this solenoid was produced by a battery of storage cells, and measured by means of a Kelvin graded galvanometer which was standardised from time to time. The maximum field was 450 c.g.s.

The Wheatstone-bridge method was used to determine the change of resistance, the connections being shown in fig. 1. P is the iron or nickel wire under investigation, Q a *comparison coil* of equal resistance. A very thick but uniform wire of German-silver was used for the bridge, but for measuring very small effects this was replaced by a nickel-plated copper wire having a much smaller resistance per unit length. A and B are two equal *auxiliary coils* of German-silver wire, immersed in an oil bath to avoid temperature differences. A Kelvin astatic galvanometer G of resistance 3·6 ohms was used in these experiments. In some preliminary measurements on the resistance change in the iron wire, the comparison resistance Q consisted of a spiral coil of the same wire. This method was first used by Gray and Jones, and theoretically it should eliminate the temperature effects due to (1) the Joule heating effect in the iron wire caused by the current which flows through it, (2) radiation and conduction of heat from the magnetising solenoid, (3) gradual changes of temperature in the whole apparatus. It was found, however, that the second of these effects still gave rise to serious difficulties, since the necessary insulation between the coils P and Q prevented rapid equalisation of temperature. Thus the transmission of heat from the solenoid affected the spiral coil Q sooner than it affected the coil P, and hence the equi-

FIG. 1.



The changes of resistance are determined as follows:—The nickel is first thoroughly demagnetised by the process of reversals; the reading x_0 of the bridge is then taken as soon as the equilibrium becomes steady. The magnetising circuit is then closed, and after reversing the current a number of times to ensure reaching a point on the curve of ascending reversals, the new reading x is taken. Finally, the magnetising current is broken, and the residual reading x' determined. Then $\Delta x = x - x_0$ is the "step" on the bridge wire due to the induced magnetisation, and $\Delta x_r = x' - x_0$ is the corresponding residual step. In taking the readings, the sliding contact piece is moved along the bridge until the point is reached at which no *permanent* deflection of the galvanometer needle takes place when the bridge current is made or broken. This was found to be the most satisfactory way of eliminating the thermo-electric effects in the bridge wire. Before each set of readings the specimen was reduced to a neutral state by the process of demagnetisation, thus avoiding the errors due to slow changes of temperature.

The fractional change of resistance $\Delta\phi$ in the nickel wire P corresponding to the step Δx is calculated from the formula

$$\Delta\phi = \frac{\Delta P}{P} = \frac{A+B}{A.B}.\sigma\Delta x,$$

where σ is the resistance per unit length of the bridge wire. A slight correction is required for the resistance of the copper leads connecting P to the bridge.

A preliminary experiment was made to test the effect of the magnetic field on the resistance of the copper comparison coil Q. The nickel coil was disconnected from the bridge, an equal resistance of German-silver being substituted. Only the copper coil Q remained in the solenoid, and as no change of resistance was observed, it was concluded that in copper the effect is negligible.

The magnetisation curves were obtained by the ballistic method. Seven pieces of the nickel wire, each 65 cm. long, were enclosed in a thin glass tube, round the middle of which was wound a ballistic coil of 200 turns of fine copper wire. The glass tube was placed in the axis of the magnetising solenoid, and the coil connected to a ballistic galvanometer provided with telescope and scale. The galvanometer was standardised before and after each set of readings by means of a standard solenoid and secondary coil.

The true magnetic force H in the nickel is given exactly by $H = H' - NI$ where H' is the magnetic field, calculated from the known constant of the solenoid and the strength of the magnetising current, and N is the demagnetising factor for the bundle of wire. In both the resistance and magnetic apparatus the value of N was less than 0·0005, so that this term could be neglected.

Results.—The corresponding values of $\Delta\phi$, H, and I for the process of ascending reversals of magnetisation in nickel, iron, and steel are contained in Table II. The change of resistance is always an increase, and is about eight times greater in nickel than in iron or steel for the same field. If we take $\Delta\phi$ as ordinate and H as abscissa, the curves obtained will be found to have the same characteristics as the ordinary magnetisation curves, except that the points of inflection are not so strongly marked. The result is quite different if I be taken as abscissa. In this case we obtain for the three metals a curve which has no point of inflection, but which resembles in general form a semicubical parabola, the curve being remarkably flat near the origin and then rising very steeply. Figs. 2 and 3 show the relation of $\Delta\phi$ to I for nickel and iron respectively, the abscissa being the *square* of the magnetisation. The curve for steel differs very little from that of iron.

For the three metals it is found that the relation $\Delta\phi = a\mathrm{I}^4$, suggested by the experiments of Gray and Jones, is not generally satisfied (*i.e.*, the curves in figs. 2 and 3 are not parabolas), although it is nearly true for strong fields in the case of iron. In weak fields the variation of resistance appears to depend on a lower power of the magnetisation, and in order to represent the results for the three metals by the same formula it was found necessary to adopt the more general expression

Fig. 2.

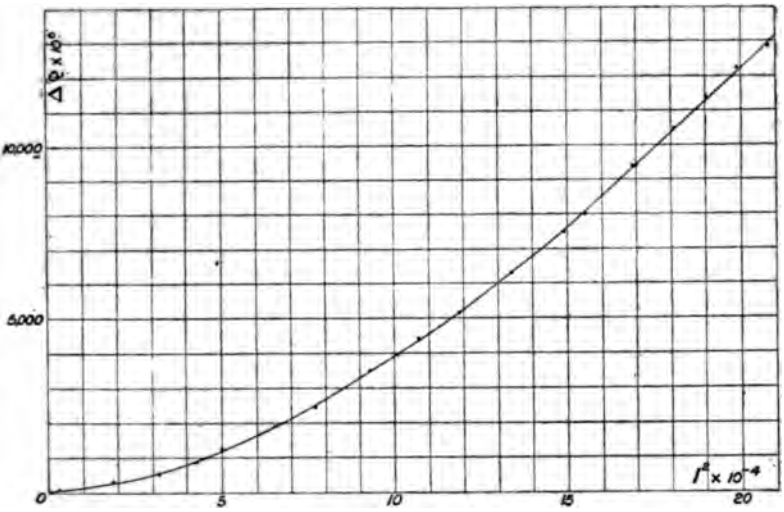
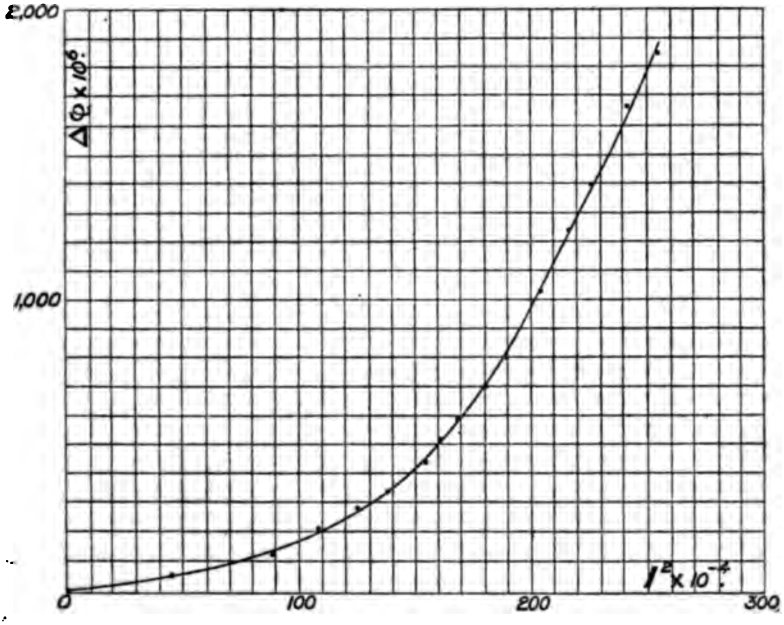
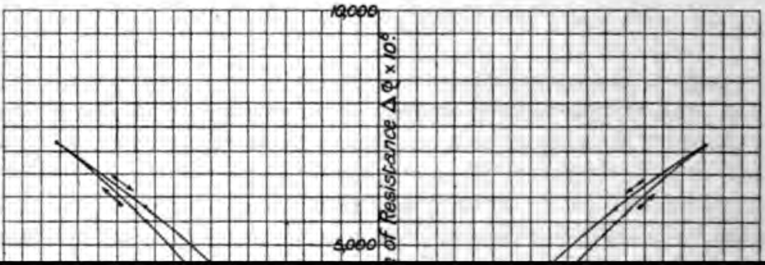


Fig. 3.



and, after inflection, attains its highest point with the maximum field $H = -165$. The loop is completed by a similar curve as the field changes from -165 to $+165$ units. At the two minimum points the curve appears to *touch* the axis. The magnetic hysteresis loops were also obtained for exactly the same cycle, and a comparison of the two sets of curves indicates that the change of resistance is very closely related to the magnetisation. Thus the "minimum points," described above, appear to be identical with the points where the magnetisation loops *cut* the axis; or in other words, when the field is such as to reduce the magnetisation to zero, the change of resistance also vanishes. Thus this critical value of the field is approximately equal to the "coercive force" of the specimen. In all cases the curves represented the cyclic state that is obtained after a large number of reversals of the field.

Fig. 4.



It should be mentioned that the change of length produced by magnetisation in iron and nickel is too small in moderate fields to account for the observed variation of resistance; it is, however, possible that this effect may give rise to an important correction in strong fields.

It is obvious that the method employed in these experiments completely eliminates any change of resistance which is reversed in sign by reversal of the field; but such an effect has never been observed.

EXPERIMENTS IN HIGH FIELDS.

Experiment I.—Longitudinal Effect in Nickel.

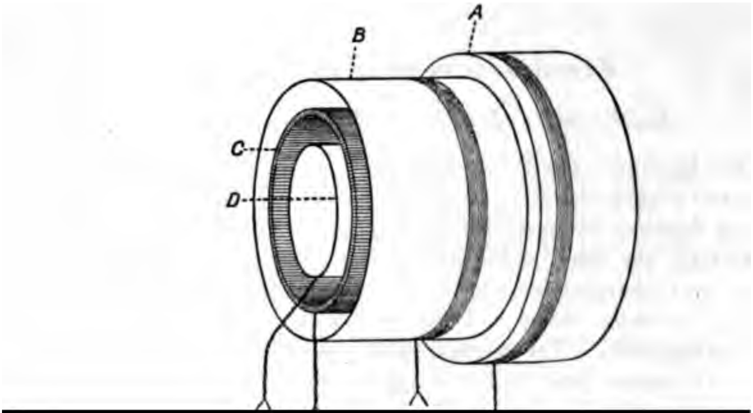
Another specimen of wire was used having a diameter of 0·33 mm., and covered with double silk insulation.

It being desirable to determine the magnetic field and magnetisation under exactly the same conditions as the change of resistance, the apparatus was arranged so as to allow the three measurements to be made with the same nickel coil. A modification of the "isthmus" method was adopted. The magnetic field was produced by means of a large electromagnet provided with conical pole pieces having faces 1·7 cm. in diameter.

The essential part of the apparatus is represented diagrammatically in figs. 5 and 6. The nickel wire was wound in a single layer, in and out the thin glass cylinder C, 1·1 cm. long and 1·1 cm. in diameter, so that the turns of the coil, 88 in number, were *parallel* to the axis of the cylinder, as shown in fig. 5. The glass being thin, the *transverse* end elements of the coil were small in comparison with the length of the cylinder. There were three ballistic coils; the innermost was wound on a glass tube D, the second on the brass cylinder B, and the outermost on the brass flange A, which extended over half the cylinder B. This system of coils was attached to one end of a wooden arm, having a pivot at the other extremity, by means of which the coils could be suddenly placed in the proper position between the poles of the magnet. Before every reading the nickel was demagnetised by a separate arrangement. The ballistic deflections as well as the resistance change were produced by placing the coils in the field, the effects of residual induction in the nickel being thus avoided. There being no compensation for temperature variations, it was necessary to make the observations as quickly as possible. It was arranged that the ballistic deflections measured the differences of the total induction through the consecutive coils. The values of H and I were then easily calculated from the two observed deflections.

As usual, it is assumed that the value of the field just outside the nickel is the same as that in the metal. The innermost ballistic coil merely served to eliminate the value of the field near the axis. method of determining I becomes insensitive when H is very gr was therefore found impossible to obtain measurements of th netisation in this case. It is probable, however, that the sat stage of magnetisation was practically reached within the range experiment.

Fig. 5.



place in the highest fields, a peculiarity that could not be accounted for by the experimental errors. In order to investigate this effect in still higher fields, the following experiment was made :—

Experiment II.—Longitudinal Effect in Nickel.

The method used was the same as in Experiment I, but the apparatus was simplified and made as small as possible. The nickel wire was wound on a glass cylinder as before, the number of complete turns being thirty-four and length of cylinder only 5 mm. A small ballistic coil was placed in the axis of the nickel coil in order to determine the field. Finer pole pieces were used and the distance between them reduced to 7·5 mm. No attempt was made to measure the magnetisation, and the field determined by the ballistic coil is assumed to be the same as that in the nickel : as this had been proved to be approximately true in the former experiment.

Results.—The change of resistance now exhibits a decided maximum—

$$\Delta\phi = 0{\cdot}0156, \qquad \mathrm{H} = 2000 \text{ c.g.s.},$$

and in higher fields decreases continuously to the value

$$\Delta\phi = 0{\cdot}0100, \qquad \mathrm{H} = 18{,}000 \text{ c.g.s.}$$

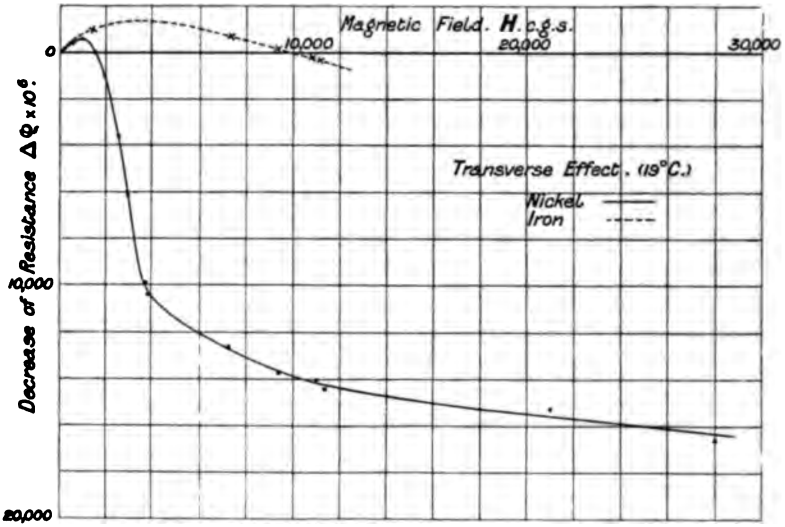
To explain this result it appears necessary to consider the effect of the end-elements of the nickel coil. In this apparatus the end-elements formed a considerable fraction of the whole coil, whereas in Experiment I this fraction was small. These elements of the wire are magnetised *transversely.* Even if there were no transverse effect in nickel, the existence of the end elements reduces the observed change of resistance, and the necessary correction cannot be estimated. But the electrical resistance of nickel is diminished by the transverse magnetisation, and this effect may therefore easily explain the peculiar results of the above experiment.

Experiment III.—Transverse Effect in Nickel.

The above experiments suggested that an examination of the transverse effect in the same specimen of nickel would prove of considerable interest. For this purpose a coil of the nickel wire was wound non-inductively on a short brass bobbin provided with wide flanges. The plane of the coil was placed at right angles to the lines of force, so that the whole of the wire was magnetised transversely. Very strong fields were obtainable on account of the small thickness of the coil, 3 mm. only. It is impossible to measure the magnetic field which exists in the metal in such a case, but the field undisturbed by the nickel coil was determined for each measurement of the resistance change. The nickel coil was subjected to the demagnetising process before each reading.

The variation of resistance is shown in fig. 7.

FIG. 7.



In weak fields, $\Delta\phi$ has a small positive value, but a reversal takes place in a field of 1500 c.g.s. For higher fields $\Delta\phi$ is negative and increases continuously; even in the highest field, $H = 28{,}000$, there is no indication of $\Delta\phi$ having a definite limit. The general form of the curve is similar to the curves obtained by Beattie for certain nickel films.

From these results it was found that the decrease observed in Experiment II might be accounted for by assuming that about one-fifth of the wire was transversely magnetised. The slight decrease observed in Experiment I is also to be explained in the same way.

Experiment IV.—Transverse Effect in Nickel.

A flat spiral of the nickel wire was inclosed between two plates of mica. Using the fine pole pieces at a distance apart of only 1 mm., the following result was obtained:—

$$\Delta\phi = -0{\cdot}0199, \qquad H = 34{,}000 \text{ c.g.s.}$$

The field strength was determined before placing the spiral in the field. This value of the resistance change is the greatest effect observed in these experiments.

Experiment V.—Transverse Effect in Iron.

An attempt was also made to measure the transverse effect in iron, ng the same method as in Experiment III. The effect observed

Table I.—Dimensions of Wires used in the Experiments.

Experiment.	Diameter of wire in millimetres.	Resistance. Ohms per centimetre. 15° C.	Actual resistance of coil in ohms. 15° C.	Total length of wire in centimetres.
dinary fields—				
Iron	0·330	0·01450	1·83	127
Steel	0·765	0·00382	1·01	263
Nickel	0·765	0·00217	0·573	264
gh fields—				
Nickel. Exp. I ..	0·200	0·02900	6·90	—
,, ,, II ..	,,	,,	1·31	—
,, ,, III ..	,,	,,	4·65	—
,, ,, IV ..	,,	,,	0·57	—
Iron ,, V ..	—	—	1·40	—

Table II.—The Longitudinal Effect for Nickel, Iron, and Steel.
(Ascending reversals for mean temperature 10° C.)

H.	Nickel.		Soft iron.		Steel.	
	I.	$A\phi \times 10^{-6}$.	I.	$A\phi \times 10^{-6}$.	I.	$A\phi \times 10^{-6}$.
10	10	—	680	50	70	—
20	60	100	1040	210	800	—
30	178	550	1180	340	1150	300
40	225	1250	1230	440	1230	470
50	256	1900	1270	520	1260	545
60	277	2450	1300	590	1280	600
80	305	3500	1340	700	1320	710
100	327	4400	1370	800	1360	823
150	366	6300	1430	1040	1420	1055
200	393	8050	1470	1240	1470	1270
250	411	9400	1500	1400	1500	1460
300	426	10500	1530	1550	1530	1640
350	436	11400	1560	1670	1560	1780
400	446	12300	1580	1760	1580	1920
450	456	12900	1590	1850	1600	2030
790*	474	} 17000				
1000*	—					
Resid. (max.) }	250	900	840	70	1120	180

Note (*) refers to Experiment I in strong fields.

was very small, but, as in the case of nickel, exhibited a change of sign, the reversal taking place in a field of 9500 c.g.s. (See fig. 7.)

The highest field obtained was only 11,000 c.g.s., and as the self-demagnetising force must be very great in this case, it is possible that with much stronger fields the effect may increase rapidly. The same specimen of iron was used as in the experiments with ordinary fields.

All the experiments described above were carried out in the Physical Laboratory of the University College of North Wales; and, in conclusion, I desire to acknowledge my great obligation to Professor E. Taylor Jones for the interest he has taken in the work, and also for much valuable help and advice.

"Influence of Temperature on the Conductivity of Electrolytic Solutions." By W. R. Bousfield, M.A., K.C., M.P., and T. Martin Lowry, D.Sc. Communicated by Professor H. E. Armstrong, F.R.S. Received and read June 19, 1902.

The phenomenon of electrolysis is characteristic mainly of the liquid the assumption that the "mobility" of the ions which carry the current is independent of the dilution, and is probably subject to serious errors when applied to solutions containing more than one equivalent of solute in 100 litres. The second method is based on the determination (usually from the boiling point or freezing point) of the osmotic pressure of the solution. The "active" part of the solute produces an osmotic pressure n times as great as that produced by an equimolecular quantity of an "inactive" solute, n being the number of ions into which the molecules of the solute would be decomposed on electrolysis. The coefficient of ionisation is deduced from the equation $a = \frac{i-1}{n-1}$, in which the factor i represents the ratio of the observed osmotic pressure to that calculated for an inactive solute at equal dilution. This method of measuring a is also valid only in dilute solutions, is probably subject to error when applied to solutions containing more than one equivalent of solute in 10 litres.

In the majority of cases the magnitude of the coefficient of ionisation is found to decrease as the temperature rises, and the primary effect of an increase of temperature is therefore to reduce the amount of active material in the solution—an effect which may be attributed to the gradual disappearance of the "ionising power" of the solvent. But whilst the coefficient of ionisation usually decreases as the temperature rises, the equivalent conductivity at infinite dilution invariably increases. This is due to an increase in the "mobilities," u and v, of the kathion and anion, which together make up λ_∞, and is intimately related to the decreasing viscosity of the solution, which allows the migration of the ions to proceed more rapidly as the temperature rises. The effect of temperature on the equivalent conductivity of an electrolytic solution is therefore determined by two opposing influences, and the temperature coefficient $\frac{1}{\lambda}\frac{d\lambda}{d\tau}$[*] will be $-$ or $+$ according as one or other of these influences predominates.

In the case of aqueous solutions, the temperature coefficient at 18° C. is always positive, and usually amounts to about 2 per cent. of the conductivity at 18° per degree Centigrade. The conductivity temperature curves are very flat (compare fig. 1), and have frequently been represented by linear formulæ such as $\lambda_t = \lambda_0 (1 + at)$, but may often be more accurately represented by a parabolic formula such as $\lambda_t = \lambda_0 (1 + at + \beta t^2)$. If the linear or parabolic curves be produced in the direction of decreasing temperature, they would cut the axis of temperature at points not more than 50° below the freezing point of water. The point of intersection is an important constant

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[*] The temperature-coefficient $\frac{1}{\kappa}\frac{d\kappa}{d\tau}$ of specific conductivity is not identical with $\frac{1}{\lambda}\frac{d\lambda}{d\tau}$; $\frac{1}{\lambda}\frac{d\lambda}{d\tau} = \delta + \frac{1}{\kappa}\frac{d\kappa}{d\tau}$ approximately, where δ is the coefficient of expansion of the solution.

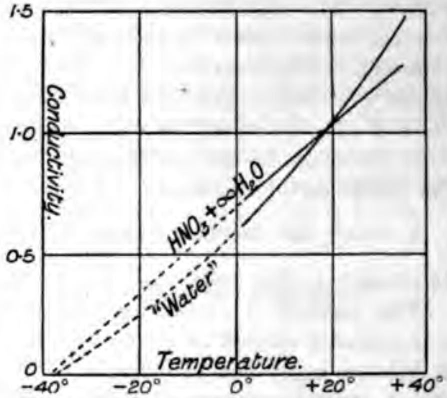
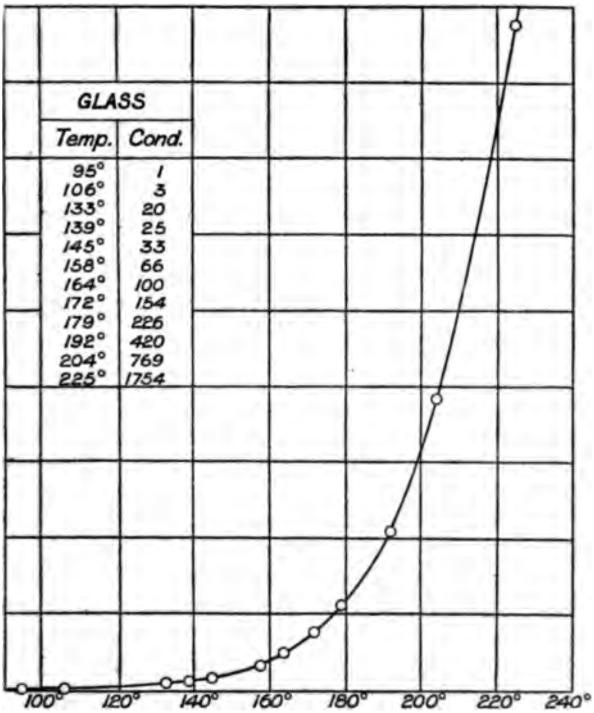


Fig. 1.—Influence of temperature on the conductivity of very dilute solutions.

for any given solution, and we have been accustomed to refer to it as the *conductivity zero* of the solution. At this temperature, if the observed relationship between conductivity and temperature should "critical temperature" the water would, "independently of crystallisation reach a lower limit of the liquid state," the mobility of the ions being simultaneously reduced to zero. The actual existence of such a sharply defined critical temperature appears to us to be somewhat improbable. We consider it more likely that in an overcooled solution the relationship between conductivity and temperature, determined from observations above the freezing-point, would cease to hold good in the neighbourhood of the conductivity zero, and that even at considerably lower temperatures the electrolyte might still retain some appreciable conductivity.

We have obtained experimental evidence in support of this view from a study of the conductivity of glass, which may be looked upon as a typical example of an overcooled electrolyte. The accompanying curve and table (fig. 2) represents a series of observations which we have made on the change with temperature of the conductivity through the walls of a glass test-tube. The test-tube was half-filled with mercury, and was placed inside a larger tube, also containing mercury, and heated in a sand-bath; the inner tube contained a thermometer,



Temp.	Cond.
95°	1
106°	3
133°	20
139°	25
145°	33
158°	66
164°	100
172°	154
179°	226
192°	420
204°	769
225°	1754

Fig. 2.—Influence of temperature on the conductivity of glass.

and the conductivity between the two layers of mercury was dete mined by a potentiometer method; the conductivities are expressed arbitrary units. Above 200° the curvature is very slight and the cur is similar to that of an aqueous electrolyte above 0°. Extrapolati from this part of the curve would indicate the existence of a "condu tivity zero" at about 185° C., but as this temperature is approach the decrease of conductivity becomes much less rapid, the "critic temperature" is passed without any abrupt change in the curve, a even below 100° the conductivity is still measurable.

Further evidence of a similar character is supplied by the temper ture-viscosity curves. The formula given by Kohlrausch leads to limit of fluidity at − 38°·5, but no statement is made as to the expe mental data on which it is based. Thorpe and Rodger* give tv formulæ for the viscosity of water. The first,

$$\eta = 5{\cdot}9849(43{\cdot}252 + t)^{-1{\cdot}5423},$$

expresses the experimental data from 0° to 100°, and would lead to limit of fluidity at − 43°·2, a temperature within 5° of Kohlrauscl critical temperature. But the second formula,

$$\eta = 58{\cdot}7375(58{\cdot}112 + t)^{-1{\cdot}9944},$$

$$\kappa_t = \kappa_{18}\{1 + \alpha(t - 18) + \beta(t - 18)^2\}.$$

In the case of solutions of very great dilution, the coefficient β, which determines the curvature of the lines (fig. 1) is related to the coefficient α, which determines the slope of the tangent at 18° C., in accordance with the equation

$$\beta = 0{\cdot}0163(\alpha - 0{\cdot}0174),$$

and the whole influence of temperature on conductivity is thus determined by a single arbitrary constant. A similar equation is also used by Kohlrausch to express the influence of temperature on the separate ionic mobilities. Negative values of β occur only in the case of the hydrogen ion,

$$\alpha = +0{\cdot}0154, \qquad \beta = -0{\cdot}000033,$$

and the acids which contain that ion; positive values of β range from that of the hydroxyl ion,

$$\alpha = +0{\cdot}0179, \qquad \beta = +0{\cdot}000008,$$

which obeys a law which is almost linear to that of the lithium ion,

$$\alpha = +0{\cdot}0261, \qquad \beta = +0{\cdot}000155,$$

where the curvature of the lines in the conductivity temperature diagram is very pronounced. In order to make a direct comparison between conductivity and viscosity—a comparison which is hardly possible when two entirely different types of formula are used for the two phenomena—we have sought to express the viscosity of water by means of the formula

$$\eta_{18} = \eta_t\{1 + \alpha(t - 18) + \beta(t - 18)^2\}.$$

In order to make the comparison as exact as possible, we have deduced the constants α and β from the values given by Thorpe and Rodger for the viscosity of water at 5°·45, 13°·52, and 30°·72, the temperature limits being thus almost identical with those of Déguisne's experiments, and have thus obtained the values

$$\alpha = +0{\cdot}0251, \qquad \beta = +0{\cdot}000115,$$

for the two constants. We find that between 5° and 55° this expression represents the experimental data with even greater closeness than the formula used by Thorpe and Rodger, but that above 55° it gives values smaller than those determined experimentally. The closeness of the agreement is shown by the following table:—

t.	η (obs.).	η (calc.) (Thorpe and Rodger.)	Diff.	η (calc.) (Bousfield and Lowry.)	Diff.
5·45	0·01494	0·01494	0	0·01494	0
13·52	0·01181	0·01179	−2	0·01181	0
22·02	0·00955	0·00951	−4	0·00954	−1
30·72	0·00786	0·00784	−2	0·00786	0
39·32	0·00662	0·00661	0	0·00662	0
47·03	0·00576	0·00577	+1	0·00576	0
55·53	0·00501	0·00502	+1	0·00500	−1

The values of α and β in this formula for the viscosity of water a considerably larger than those which express the influence of te perature on the conductivity of the majority of aqueous solutions, b *agree remarkably closely with the values given by Kohlrausch for the co ductivity of purified water*; this he found to have an abnormally larg temperature coefficient, the constants being

$$\alpha = +0\cdot0254, \qquad \beta = +0\cdot000130,$$

as compared with the values of the constants in the viscosity formul

$$\alpha = +0\cdot0251. \qquad \beta = +0\cdot000115.$$

The striking agreement between the constants in the two equatio

In no case is the temperature coefficient of viscosity so great as in the case of water, and the conductivity zero for each of these liquids is evidently very far below the conductivity zero for water.

(2.) At great dilutions the influence of temperature on ionisation is usually much reduced, and aqueous salt solutions of N/1000 strength have temperature coefficients of conductivity which do not differ appreciably from those calculated for solutions of indefinitely great dilution. These coefficients can be used in determining the conductivity zero of the solvent, and a rough estimation is possible from measurements made in much less dilute solutions. The conductivity of a number of salts dissolved in liquid ammonia has been determined by Legrande* at temperatures ranging from − 32° to − 70° C., the dilution in each case being greater than 100 litres per equivalent. The mean temperature coefficient at − 60° C. for the five salts was + 0·012, corresponding with a conductivity zero at − 140°. A similar examination of Walden and Centnerszwer's measurements of solutions in liquid sulphur dioxide between 0° and − 70° C.† serves to place the conductivity zero for this solvent at about − 160° C. The measurements of Dutoit and Friedrich‡ of the conductivity between 0° and 25° of solutions of AmCNS and NaI in acetonitrile lead to a conductivity zero for the nitrile at − 90° C.

(3.) The temperature coefficient of conductivity of purified water leads to a satisfactory value for the conductivity zero. By applying a similar method to Fresnel's measurements of the conductivity between − 40° and − 80° of purified ammonia§ we have obtained for the conductivity zero the value − 130°, a value which is substantially in agreement with that deduced from the behaviour of Legrande's salt solutions.

Whilst at low temperatures the effect of temperature on conductivity is determined mainly by the changing mobility of the ions, at higher temperatures the changing ionisation becomes the dominant factor. And just as the increasing viscosity of the solution leads at low temperatures towards a *lower* conductivity zero at which the viscosity of the liquid would altogether prevent electrolysis, so at high temperatures an *upper* limit may exist at which the conductivity would again become zero owing to the complete disappearance of ionisation.

In the case of aqueous solutions the indications of an *upper conductivity zero* are only slight. At 18° the temperature coefficients of conductivity are all positive, and only in very exceptional cases have negative coefficients been observed, even at higher temperatures.

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* Thesis, Paris, 1900.

† 'Zeit. Phys. Chem.,' 1902, vol. 39, p. 542.

‡ 'Bul. Soc. Chim.,' 1898, vol. 19, pp. 321—337.

§ 'Zeit. Electrochem.,' 1900, vol. 6, p. 485.

Nevertheless, when the influence of changing ionic-mobility is eliminated, and the coefficient of ionisation is dealt with separately, the decay of ionisation as temperature rises is in many cases very clearly marked, even at 0° C., and in presence of a large excess of the solvent. Thus Whetham* has shown that at a dilution of 1000 litres per molecule of solute the coefficient of ionisation of potassium chloride falls from 0·992 at 0° to 0·982 at 18°, and that of barium chloride from 0·969 at 0° to 0·954 at 18°; in the case of copper sulphate no decrease occurs at dilutions greater than 1000 litres, but at a dilution of 100 litres the coefficient falls from 0·638 at 0° to 0·623 at 18°. These and other measurements that have been made between 0° and 100° serve, however, mainly to show that within these limits the coefficient of ionisation is influenced only to a relatively small extent by temperature, and that the upper conductivity zero must lie very much further from the boiling point than the lower zero is removed from the freezing point.

The different behaviour of acids and salts, which is unimportant in the case of the lower conductivity zero, becomes a vital factor in discussing the possible existence of an upper conductivity zero. Armstrong† has already called attention to the importance of the fact that whilst the majority of salts are conductors *per se*, the acids boiling point and experimental work is rendered very difficult, owing to the ready solubility of glass in water at high temperatures; this not only contaminates the solutions but greatly weakens the sealed tubes in which they are contained. Hagenbach[*] has, however, made a single observation which is of importance as showing that even the salts of the alkali-metals may exhibit negative temperature coefficients at high temperatures; in the case of an aqueous solution of KCl, containing dissolved glass, he actually observed on one occasion a maximum of conductivity before the explosion of the tube took place at 310°.

Much more evidence is available for the existence of an upper conductivity zero in the case of non-aqueous solutions. Franklin and Kraus[†] have found that at high temperatures the conductivity of solutions in liquid ammonia decreases as the temperature rises—an effect directly opposite to that observed by Legrande at low temperatures; and Maltby[‡] has shown that even at atmospheric temperatures the conductivity of an ethereal solution of hydrogen chloride decreases as the temperature rises, and in the neighbourhood of the critical temperature is only $\frac{1}{38}$ of the conductivity at 18°. Negative temperature coefficients have also been observed by Cattaneo[§] in solutions in ether, alcohol and glycerol.

The most valuable experimental data, however, are those derived from the study of solutions in liquid sulphur dioxide. Hagenbach,[||] in order to ascertain whether the limit of conductivity was reached at the critical temperature, measured the conductivity of solutions in sulphur dioxide at temperatures ranging from 20° to 160°, and found that the temperature-coefficients between 100° and 140° of KCl, KBr, KI, and NaI were all negative, and amounted to about 2 per cent. of the conductivity at 100° for each degree Centigrade. The upper conductivity zero of these solutions, determined by extrapolation from the measurements between 130° and 150°, would lie in each case some 5° or 10° above the critical temperature of the solution, but immediately below the critical temperature the conductivity falls rapidly over a narrow range of temperature until it reaches the small but measurable conductivity of the gas. It is to be noticed that the substances examined by Hagenbach are all self-ionising—this would retard the decay of ionisation, and would have the effect of raising the upper temperature-limit of conductivity. High temperature measurements have also been made by Walden and Centnerszwer[¶] in the case of solutions in sulphur dioxide of hydrogen chloride, quinoline, and a number of organic iodides. The first of these solutions is of special interest, since hydrogen chloride is known to be of itself a non-electrolyte, and to possess no power of self-ionisation; the conductivity of its solution in sulphur dioxide, unlike that of the salt-solutions examined by Hagenbach, appears to decrease regularly to a zero value at the critical temperature, which is therefore identical with the upper conductivity zero of the solution.

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[*] 'Ann. d. Physik,' 1901 [5], vol. 2, p. 306.

[†] 'Amer. Chem. Jour.,' 1900, vol. 24, p. 83.

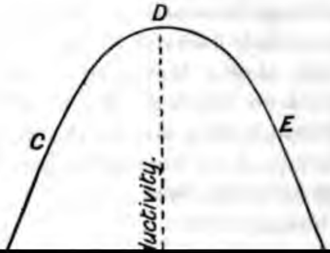
[‡] 'Zeit. Phys. Chem.,' 1895, vol. 18, p. 133.

[§] 'Rend. Lincei' [5], vol. 2, I, p. 295, and II, p. 112, 1893.

[||] 'Ann. d. Physik,' 1901 [5], vol. 2, pp. 276—312.

[¶] 'Zeit. Phys. Chem.,' 1902, vol. 39, p. 549.

We are now in a position to review the influence of temperature on the conductivity of a "composite electrolyte" over the whole range of temperature within which it remains a conductor. Its general character may be represented by means of a curve (fig. 3), in which tempera-



lower conductivity zero of the solution; but as this temperature is approached the curve probably turns aside and becomes asymptotic to the axis of temperature.

Above the temperature of maximum conductivity the conductivity also decreases, the decreasing viscosity being now more than counterbalanced by the decreasing ionisation of the solution. The decay of ionisation becomes more rapid as the temperature rises, and if the solute is not an electrolyte *per se*, the curve EF runs steadily down and cuts the axis at T_4, the critical temperature of the solution, which is thus the upper conductivity-zero of the solution. In the case of very dilute solutions this would, of course, be identical with the critical temperature of the solvent. If, however, the solute is capable of self-ionisation, the curve EFG tends towards an upper conductivity zero at T_5 a few degrees above the critical temperature, but as the critical temperature is approached the conductivity falls abruptly along GH to a value comparable with that which persists in the gaseous state HI.

[*Note added August* 2, 1902.—The general scheme of fig. 3 serves to bring into prominence at least one important point that has been very generally overlooked, namely, that *the normal form of the conductivity-temperature curve for a composite electrolyte is one which contains a point of inflexion.* Two such points are shown in fig. 3, between B and C and between E and F. Of these, the former should be frequently observed in aqueous solutions, seeing that these give values lying on parts of the curve both above and below the inflexion. A widespread impression exists, however, that an inflexion in the conductivity-temperature curve indicates some abnormal change in the character of the solution. This impression has been strengthened, if indeed it has not been created, by the general adoption of a linear or parabolic formula to express the influence of temperature on conductivity. Kohlrausch, in his detailed review of the literature of the subject,* makes no reference to the existence of inflected curves, and does not even hint at the possibility of curves of this type. Trötsch, who observed inflexions in the case of a number of sulphates and the chlorides of copper and cobalt,† regarded them as due to the decomposition of hydrates existing in the solution, whilst Donnan and Bassett, in a paper which has only just appeared,‡ quote the inflexion observed by Trötsch as evidence for the existence of a complex ion in solutions of cobalt chloride.

According to the views here put forward, the conductivity-temperature curves are all inflected. In the case of the acids, which owe their conducting power entirely to the action of the solvent, the inflexion lies below 0°; in the case of the alkalies our own experiments have shown that the curves, which have usually been represented as straight lines or flat parabolas, all exhibit inflexions, in the case of the alkalies, LiOH, NaOH and KOH at about 40°, and in the case of the alkaline earths, $Ca(OH)_2$, $Sr(OH)_2$ and $Ba(OH)_2$, at about 25°; in the case of the less highly ionised salts, such as magnesium sulphate, the inflexion also lies below 100°, but in the case of salts such as potassium chloride, which have a high coefficient of ionisation in solution and are also electrolytes *per se* in the fused state, the inflexion lies above the boiling-point of the solution.]

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* 'Leitvermögen der Elektrolyte,' pp. 116–123 and 195–199.

† 'Ann. Phys. Chem.,' 1890 [iii], vol. 41, pp. 259–287.

‡ 'Jour. Chem. Soc.,' vol. 81, p. 953, August, 1902.

"On the Measurement of the Bactericidal Power of Small Samples of Blood under Aerobic and Anaerobic Conditions, and on the Comparative Bactericidal Effect of Human Blood drawn off and tested under these Contrasted Conditions."

By A. E. Wright, M.D., Professor of Pathology, Army Medical School, Netley. Communicated by Professor J. R. Bradford, F.R.S.

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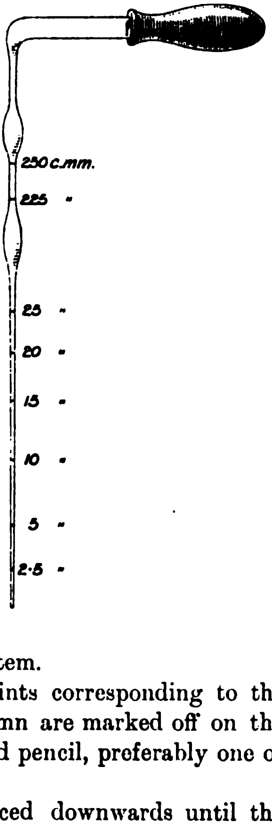
measured dilution of the culture (in the case of an ordinary bacterial culture, dilutions of 1,000,000 fold to 10,000,000 fold are appropriate), and, *secondly*, the transference—with a view to the subsequent enumeration of the colonies which develop—of a series of measured volumes of the diluted culture to the surface of a solid nutrient medium.

The processes of diluting and measuring off the desired volumes of diluted culture can be conveniently and unlaboriously carried out by means of the diluting pipette figured below. (Fig. 1).

Fig. 1.



(1.) *Method of making and calibrating the "Diluting Pipette."*

(*a.*) A piece of glass tubing about 15 cm. long is drawn out at one end into a capillary stem.

(*b.*) A standard 5-cm. pipette is fitted with a rubber teat,* and is then filled in up to the calibration mark with mercury. The 5 cm. of mercury is now transferred from the capillary pipette to the wide end of the glass tube, and is made to enter the upper portion of the capillary stem.

(*c.*) When this has been effected, the points corresponding to the upper and lower ends of the mercury column are marked off on the outside of the capillary stem with a coloured pencil, preferably one of the oil pencils sold for writing on glass.

(*d.*) The mercury column is now displaced downwards until the upper end of the column stands opposite the lower of the calibration marks. This point is again marked off with the coloured pencil.

(*e.*) By a similar procedure, three more marks indicating divisions of similar value are placed on the outside of the capillary stem.

(*f.*) This done, the tube is filed and broken off at the lowest mark.

(*g.*) It will now be convenient to divide the lowest 5-cm. division into two divisions of 2·5 cm. This can be conveniently effected by the following method of trial and error :—Mercury is drawn up into the tube until the upper limit of the column of metal stands as nearly as possible midway between the orifice and the first 5-cm. division mark. The point corresponding to the proximal end of the mercury column may now be tentatively indicated on the outside of the capillary stem by a light pencil mark. This done, the column of mercury is displaced until its proximal end stands level with the 5-cm. division mark. If the distal end of the mercury column now coincides with the tentative subdivision mark, this last gives the desired 2·5-cm. division. If it does not coincide, the desired point will be situated half-way between the point now indicated by the proximal extremity of the mercury column and the point indicated by the original trial subdivision mark.

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* A mechanically controlled teat, such as that made by Mr. A. E. Dean, jun., 73, Hatton Garden, E.C., is a convenient *form to* employ for this purpose.

(*h.*) What has been achieved up to this point is a graduation of the capillary stem into five divisions of 5 cm., and a subdivision of the first of these into two 2·5-cm. divisions. A further process of graduation in terms of 25 cm. is now taken in hand, with a view to finding the points corresponding respectively to 225 and 250 cm.

(*i.*) For this purpose a rubber teat is placed upon the upper end of the tube, and a negative pressure having been established, the capillary stem is filled up to the 25-cm. mark with mercury, water, or a coloured fluid. It is then filled in succession with eight further 25-cm. volumes, the 25-cm. volumes being in each case spaced off from each other by a bubble of air. After these air bubbles have risen to the surface in the wide upper portion of the tube, and the separate volumes have here

A *ten-fold dilution* of the culture—the dilution which is perhaps most often required—is made by taking first 25 cm. of the culture, and then, after the interposition of an air bubble, filling up to the 250-cm. mark with sterile broth. It can also, and this avoids any contamination of the sterile diluting fluid, be made by filling up first with 225 cm. of the broth, and completing up to the 250-cm. mark with the culture.

A *six-fold dilution*, should such be required, would be obtained by filling in to the 250-cm. mark with sterile broth, and then completing with two volumes of culture, these last being isolated as before by intervening air bubbles.

A *five-fold dilution* is obtained by filling in with two separate 25-cm. volumes of the culture, and completing up to the 250-cm. mark with sterile broth.

A *two and a half-fold dilution* would be obtained by filling in with four separate volumes of 25 cm., and completing up to the 250-cm. mark with sterile broth.

Dilutions of a different order can be obtained by filling in the pipette as occasions may require with 2·5 or 5 cm. of culture, and then completing to 250 cm. with sterile broth. By this means dilutions of 1 in 100 and 1 in 50 respectively can be obtained *uno saltu.*

By a series of successive dilutions, made in each case after washing out the pipette with boiling sterile water, any desired attenuation of the culture can be quickly arrived at. The dilution of 1 in 1,000,000 ordinarily required for the purposes of enumeration will be obtained by three successive dilutions of 1 in 100.

(3.) *Method of Eliciting the Number of Micro-organisms contained in the Diluted Culture.*

The required dilution of, let us say, 1 in 1,000,000 having been prepared, the pipette would, after sterilisation in boiling sterile water, be filled in with, say, three successive 10 cm. volumes of the diluted culture. A corresponding number of agar tubes having been taken in hand, the three 10 cm. volumes of diluted culture would be separately transferred to the surface of the nutrient medium, care being taken in each case to spread out the fluid over as large an area of surface as possible.

After incubation, the number of bacteria in each 10 cm. volumes of diluted culture would be deduced from the number of colonies which develop on the corresponding agar tube.

After averaging the number of bacteria contained in the three tubes, the number of living bacteria contained in 1 c.c. of the original culture would be found by a simple arithmetical process.

II. *Procedures in connection with the actual carrying out of the Bactericidal Estimation.*

In connection with the actual carrying out of the bactericidal estimation, we have to consider :—

(1.) The collection of the sample of blood for examination.

(2.) The preparation of a graduated series of dilutions of the bacterial culture.

(3.) The special form of capillary-testing pipette required for the subsequent procedures.

(4.) The method of employing the testing pipette just mentioned, *i.e.*, the method of mixing a series of measured volumes of serum with in each case an equal volume of the successive bacterial dilutions, and the method of determining the sterility or otherwise of the mixtures after the serum has acted upon the bacteria for an appropriate period.

(1.) *Collection of the Sample of Blood.*

The quantity of blood required for an ordinary bactericidal estimation need never exceed 1 c.c.* Much more than the quantity required can, in the case of man, readily be obtained by driving the sufficient blood has been collected, the upper portion of the capsule is gently warmed and the upper orifice is then immediately sealed up. As the air, which has been rarefied by warming, contracts, the blood is drawn up into the body of the capsule, leaving the orifice at (B) free for resealing. After the capsule has cooled, it is suspended by means of its curved arm in a hand centrifuge, and the blood is driven down by a few turns of the handle into a previously upper half of the capsule. It is now left at rest for a few minutes. When the serum begins to exude, the centrifuge is again brought into action.

When the estimation is to be taken in hand, the neck of the curved bulb of the capsule is sterilised in the flame and is cut across with a stout pair of bone forceps, which has been sterilised in the same manner.

Two further points in connection with sample of blood may appropriately be considered. The first of these relates to the question of the necessity for aseptic precautions in drawing off the blood. The second to the question of the interval which may elapse between the drawing off of the blood and the bactericidal estimation.

The blood should be drawn off with aseptic precautions. For this purpose the surface of the finger may be readily and effectually sterilised by moistening it with alcohol, and burning this off.

Where results which are comparable among themselves are desired, the bactericidal estimations ought, in all cases, to be undertaken within a very few hours after the samples of blood have been withdrawn. Where samples of blood are tested immediately after withdrawal, and again after an interval of 24 hours, it is usual to find a notable diminution of bactericidal power in the second estimation.

(2.) *Preparation of a Series of Graduated Dilutions of the Bacterial Culture.*

It has already been indicated, but it will be well at this point clearly to bring out the fact, that in the method of bactericidal estimation here described, a series of measured volumes of undiluted serum are brought in contact with a series of graduated dilutions of the culture, the object being to determine what is the lowest dilution of the culture with which a complete bactericidal effect is exerted.

The graduated dilutions of the culture which are required for this purpose, so far as they have not already been provided by the procedure undertaken in connection with the enumeration of the bacterial culture, would, at this stage, be prepared by the aid of the diluting pipette. I have found it convenient in the case of the typhoid bacillus to employ, in addition to the undiluted culture, in each case a 2 fold, 5 fold, 10 fold, 25 fold, 50 fold, 100 fold, 1000 fold, 10,000 fold, and 100,000 fold dilution.

The dilutions are to be held ready for use in a series of covered sterile watch-glasses.

(3.) *Description of the Special Form of Capillary Testing Pipette Employed in the Connection with the Method of Bactericidal Estimation here described.*

FIG. 4.

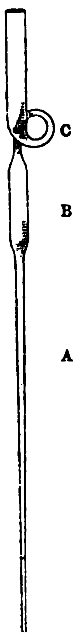

Fig. 4 shows the particular form of capillary testing pipette which has been found most suitable. The stem A—it will be noted that it is provided with a pencil mark—serves on the one hand as a measuring pipette for measuring off equivalent volumes of serum and bacterial culture, and on the other hand as a receptacle for the combined volumes of fluid during the period allotted to the action of the serum upon the culture.

The bulb B functions, in the first stage of the procedure described below, as a receptacle for the sterile broth afterwards used for testing the continued vitality or otherwise of the bacteria which have been exposed to the influence of the serum. In the second part of the procedure, the bulb of the pipette comes into use as a cultivation chamber. The bulb may conveniently possess a capacity of about 1 c.c. The spiral C* serves to prevent any access of contaminating bacteria to the interior of the bulb.

(4.) *Method of Employing the Capillary Testing Pipettes.*

The method of employing the testing pipettes is as follows:—

A mark with the coloured oil pencil having been placed upon the capillary stem at any convenient point, say at a point 1—1·5 cm. from the lower end, a rubber teat is fitted over the upper end of the tube.

The point of the capillary stem is now broken off between finger and thumb, the lower portion is sterilised in the flame, and the air is expelled from the teat.

Sterile broth, which has been placed ready to hand in a covered sterile watch-glass, is then aspirated into the pipette until the bulb is about two-thirds full.

The extremity of the capillary stem is now withdrawn from the broth, and the column of fluid which occupies it is allowed to run up very gently, so as to avoid any back lash, into the bulb. The inflow of air is arrested as soon as the capillary stem is empty of fluid.

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* It will be found that the introduction of the spiral avoids the necessity for the troublesome plugging of the tube with cotton wool and the subsequent sterilisation process. The very simple trick of hand by which the spiral is made may be readily learned by imitating the motions associated with the making of a similar spiral upon a stiff cord or a very pliable wire.

(a.) *Method of Measuring off and Mixing together equal Volumes of Serum and Bacterial Cultures.*

The end of the capillary stem is now inserted into the narrow open end of the blood capsule, which has been placed ready to hand in a perforated rubber bung or other convenient receptacle. The serum is allowed to flow in until it reaches the pencil mark.

The orifice of the pipette is now raised above the surface of the serum, and a bubble of air is admitted into the tube to serve as an index for the next measurement.

This done, the end of the capillary stem is carried over into a watch-glass containing the particular dilution of the culture which is to be dealt with in this particular tube. The culture is allowed to flow in until the bubble of air has just been carried past the pencil mark.

The next procedure is to mix together the equal volumes of serum and culture which have been measured off. This is effected by blowing these two volumes out upon the surface of a sterile watch-glass—a pile of inverted sterile watch-glasses will for this purpose have been placed ready to hand—and drawing up and driving out the fluid several times in succession. After a little practice* this can be quite easily achieved without driving the sterile broth down from the bulb of the pipette into the lower part of the capillary stem and there contaminating it.

The column of mixed serum and culture is to be drawn up into the middle region of the capillary stem as a preliminary to sealing the lower end of the tube. It will be found that when the column is left in this position, the intervening column of air which occupies the upper portion of the capillary tube will effectually isolate the fluid in the bulb of the pipette for the mixture of serum and culture.

The teat is now removed, leaving the spiral to guard the contents of the tube against contamination, and the filling of the series of tubes with the remaining dilutions of the culture is proceeded with. When the whole series of tubes has been filled in, these are placed upright in a test tube labelled with the date and the source of the serum. The serum is then allowed to exert its influence on the bacteria with which it has been brought in contact for a fixed period at a fixed temperature.

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* Until practice shall have conferred sufficient control over the teat, it will be advisable either to employ very fine capillary tubes or to provide a by-channel for the air by piercing the teat with a spicule of an extremely fine capillary tube.

I have found it convenient to allow the serum to remain in con
with the culture for a period of 18 to 24 hours at 37° C.

(b.) *Method of Testing the Continued Vitality or otherwise of the Bac*
which have been in Contact with the Serum.

The sterile broth which has been filled into the capillary pip
furnishes, as we have seen, the means for determining whether
bacteria which have been brought in contact with the serum hav
have not retained their vitality. If the serum has failed to kill
bacteria, this will be evidenced by the development of turbidity in
broth which will follow upon the aspiration of the column of flui
the capillary stem into the bulb of the pipette. If, on the other h
the serum has killed all the bacteria with which it has been mi
the nutrient broth will, under the circumstances, remain clear.

The steps of the procedure are as follows:—

The tubes having been taken in hand singly, the lower portion of
capillary stem is in each case drawn out, and after heating in a p
flame, into the finest possible filiform tube.

A condition of negative pressure is now established in the inte results of bacterial enumerations carried out at a series of successive intervals upon one and the same mixture of serum and culture. The fallacy just referred to comes in in connection with the circumstance that all evidence of a bactericidal effect exerted will be obliterated if the intervals between the successive enumerations happen to be such as to allow of the covering up of losses due to the bactericidal action of the serum by a subsequent multiplication of the surviving micro-organisms.

A further point which has been kept in view in designing the above method, is the importance of obtaining a simple numerical expression for the bactericidal power of the blood.

Such a simple numerical expression is obtained by specifying the number of bacteria contained in 1 c.c. of the lowest dilution of the bacterial culture which is completely sterilised by digestion with an equal volume of serum.

While a convenient basis for the comparison of the bactericidal power of a series of different bloods is thus provided, it must be understood that the expression just referred to is nothing more than an arbitrary formula expressing the bactericidal effect of the serum brought into application in the form of a 50 per cent. solution.

If it is desired in any case to determine the bactericidal effect exerted by the serum in a practically undiluted condition, this can readily be achieved by making a graduated series of dilutions of the enumerated culture, using the serum itself as a diluent.

In concluding this section it will perhaps not be amiss to point out that the method of bactericidal estimation here described may be employed not only for determining the bactericidal power of the blood, but also for determining that of any chemical antiseptic.

SECTION II.—*Method of Measuring the Bactericidal Power of the Blood under Anaerobic Conditions.*

The method of measuring the bactericidal power of the blood under anaerobic conditions which is here to be described, is similar to the method described in the previous section, except in so far as the technique is modified with a view to excluding the air from contact with the blood.

Access of air is prevented by enveloping the blood in oil.

It is essential that this oil should be absolutely neutral, first, because the presence of fatty acid might affect the bactericidal power of the serum by diminishing its alkalinity and by precipitating its calcium salts, and, secondly, because an oil containing fatty acids is emulsified when it is brought in contact with serum, nutrient broth, and alkaline fluids generally. Such an emulsification would interfere with that sharp separation of the oil from the enclosed fluids which is absolutely essential to the proper carrying out of the technique desc below.

It will be well, therefore, to commence by describing the m adopted for the preparation of a fatty acid-free oil.

Method adopted for obtaining a Fatty acid-free Oil.

The method I have employed is a modification of that whicl employed by the late Prof. E. Külz for obtaining a fatty acid-fr for experiments in connection with pancreatic digestion.

The procedure will perhaps be most clearly described by det an actual experiment.

300 c.c. of a cheap variety of table oil (cotton oil ?)* was intro into a litre flask along with 150 c.c. of half saturated barium hy solution. These fluids were digested together at 60° C. on a bath for three hours, the contents of the flask being well shak at intervals.

After this time the contents had separated into three laye upper layer of more or less clear oil, a middle layer, about half ai deep, of barium soaps, and a lower layer of barium hydrate solut

A drop of the supernatant oil was now tested by shaking it bottle and is kept sheltered from light. Before it is employed for the purposes described below, it is sterilised by heating to 140° C. in a test-tube, and is each time re-tested by shaking up with the dilute sodium carbonate solution.

Procedure adopted for obtaining from the Finger a Sample of Blood without allowing this to come in contact with the External Air.

A receptacle for the blood is first provided by drawing out a test-tube to form such a " thimble " as is represented in fig. 5 (p. 66).

The thimble is filled in with sterilised fatty acid-free oil, and is covered in with a sterilised cover glass.

The ulnar aspect of a finger—preferably of the little finger of the left hand—is now sterilised by flaming alcohol. It is then punctured in two or three adjacent points by a fine spicule of glass. A clean handkerchief is wound round the digit, the tip of this last is immersed into the oil, and pressure is applied to the finger pulp. The blood as it emerges descends through the oil in the form of large globules.

When pressure on the pulp ceases to yield blood, the finger is momentarily removed from the oil, the handkerchief is loosened and re-applied, the finger is re-immersed into the oil, and pressure is again made on the finger pulp.

When a sufficiency of blood has been collected, a sterilised rubber test-tube cap is drawn over the thimble. This last is then placed in a hand-centrifuge, and the blood is, by a few turns of the handle, driven down to the lower narrow end of the tube.

After allowing an interval of 10 minutes to elapse, the centrifugalisation of the coagulated blood—the blood, it may be noted, invariably coagulates*—is taken in hand. The contents of the thimble will now arrange themselves into an upper layer of oil, a middle layer of clear serum, and a lower layer of blood corpuscles.

With a view to ensuring the asepticity of the further procedure, the serum may now with advantage be separated from the oil in the thimble, which has been exposed to some risk of aerial contamination.

Procedure for the Separation of the Serum from the Oil in the Thimble

The procedure is as follows :—

A series of three or four tubes, fig. 6, which are to function respectively as receiving and mixing tubes, are flamed, filled in with sterilised oil, horizontally inclined, and placed ready to hand. A capillary testing pipette, similar to that figured (fig. 4) and described n the first section of this paper, is fitted with a rubber teat; sterile oil is now aspirated into the pipette to replace the air in the capillary stem and the lower part of the bulb. When this replacement has been effected, the tip of the pipette is thrust down through the oil contained in the thimble into the layer of serum, and this last is aspirated into the pipette. When this has been accomplished, a little

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* (Added note.) The common text-book statement that coagulation is suspended when blood is collected under oil is, it may be presumed, based on experiments undertaken with oil containing free fatty acids. A decalcification of the blood might under such circumstances result.

FIG. 5.

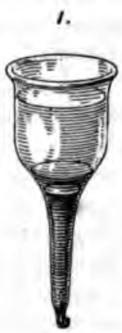
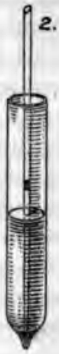


FIG. 6.



carried across to the vessel which contains the culture, and is filled in with this last up to the mark on the capillary stem.

Mixture of the serum and culture can now be effected either (*a*) in the pipette itself, or—and I owe this suggestion to my colleague, Major W. B. Leishman, R.A.M.C.,—(*b*) in a mixing tube such as is shown in fig. 6.

In the former case, oil in sufficient quantity to seal the lower end of the column of fluid is aspirated into the pipette, after the volumes of serum and culture have been measured in in the manner described. The contents of this tube are then cautiously drawn upwards until the walls of the capillary stem fall away sufficiently to liberate the index globule of oil. This obstacle having been got rid of, a series of upwards and downwards displacements of the combined column of fluid will bring about the desired mixture.

In the case where a mixing tube is employed, the extremity of the capillary pipette is carried down to the floor of the mixing tube, and the contents are driven out under the covering seal of oil. They are then intermixed by alternately drawing them in and driving them out of the capillary stem, care being taken that the pipette is never emptied of oil. Lastly, the mixed fluids are carefully and completely reaspirated from the floor of the narrowed end of the tube, the inflow being in each case allowed to continue until a sufficient seal of oil has been carried in behind the mixture of serum and culture. As soon as this has taken place, the point of the pipette is withdrawn from the oil, and air is allowed to enter and occupy the lower third of the stem. Finally, the orifice is sealed in the flame.

In filling in a succession of capillary pipettes from one and the same mixing tube, it will, as consideration will show, be advantageous to begin with the highest dilution, and to follow on in order with the lower dilutions.

Procedure for determining the Bactericidal Effect exerted by the Serum in the absence of Air.

The process of filling in the bulb of the capillary pipette with sterile nutrient broth—a process which is, in the case of the ordinary aerobic procedure described in the previous section, undertaken as a first step in the filling in of the pipette—is, in the case of the anaerobic procedure, undertaken as the final procedure after the serum and the culture have been in contact for the desired period.

It is carried out in the following way:—

Sterile nutrient broth having been placed ready in a covered watch-glass, the capillary pipette which contains the highest dilution of the culture is taken in hand. A negative pressure having been established in its interior by fitting on a collapsed rubber teat, the lower portion of the stem is passed through the flame of a peep-light in such a as to heat it without allowing it to fuse and to collapse unde influence of the internal negative pressure. The sterilised extre is now snapped off by plunging it while still hot into the nut broth. The inflow which takes place through the orifice thus vided is arrested by the pressure of the finger and thumb upo teat as soon as the cultivation chamber is about two-thirds full.

The sealing up of the tube and the subsequent cultivation carried out in exactly the same manner as in the case of the ordi (aerobic) estimation described in Section I.

SECTION III.—*On the Bactericidal Effects produced by one and the Human Blood* (a) *Drawn off and Tested by the Aerobic Proce described in Section I; and* (b) *Drawn off and Tested by the Anae Procedure described in Section II.*

In view of the fundamental theoretical importance which attach the assumption that the bactericidal power of the blood is acqu only after withdrawal from the organism, and, in particular, afte disintegration of the leucocytes under the influence of air and con with the wall of unoiled or unparaffined receptacles, it see ed in Section I; an

illed in with –		
E. W.'s serum. of a typhoid e, containing 0,000 T.B. per		**1 vol.** **1 vo** **cult** **220** **c.c.**
: 'e.	Anaerobic procedure.	**Aero** **proce**
	—	—
.	Growth	—
	"	—
	"	**Grow**
	"	**"**
	"	**Ste**
	"	**"**
	"	**"**
	"	**"**
	"	**"**

tures. The serum **was**

"The Colour-physiology of Higher Crustacea."

By FREDERICK KEEBLE, M.A., Reading College, Reading, and F. W. GAMBLE, D.Sc., Owens College, Manchester.

Communicated by Professor S. J. HICKSON, F.R.S. Received July 16, 1902.

(Abstract.)

The following statement is a condensed summary of the results of a research into the form and physiology of the pigment-bearing organs (chromatophores) of certain Schizopod and Decapod Crustacea with especial reference to the effect of light on these organs and on these animals. The evidence for the statement will appear in a full and illustrated form in the "Philosophical Transactions." The Grant Committee of the Royal Society allotted £25 for this research.

A. *The Influence of Light.*

1. Under the influence of light the secretory activity of certain organs is modified: an acid substance appears periodically in the "liver" and muscle: the appearance and disappearance of acid substance in liver and muscle coincides broadly with nocturnal and diurnal colour-change.

2. In the progressive movements and orientations of the whole animal called forth by light, background is the most important factor: more powerful than change of light-intensity. By change of background, black to white, the direction of a light-induced movement may be reversed.

3. The response of the chromatophore-pigments to light is two-fold: direct; and indirect, through the mediation of the eye. The indirect response alone leads to an enduring redistribution of pigment.

4. The ultimate effect of monochromatic light on pigment-movement is the same as that of white light. As with the latter, so with monochromatic light, background—white (scattering), black (absorbing), mirror (reflecting)—determines the nature and extent of the pigment-movements. In describing an effect of light, that light must be considered in combination with its background. Neglect to do this must lead to erroneous conclusions.

5. "Reaction to background" is traceable to the eye, and is probably a consequence of an asymetrical distribution of retinal pigment brought about not by changes in the amount of light falling on the eye, so much as by changes in the way in which light falls on the eye.

B. *The* Rôle *of Pigments.*

6. The phenomena presented by the pigments are not exhaustively explained by any "protective" hypothesis.

The chromatophores are centres of metabolic activity, and from them nocturnal translocation of a blue substance takes place. There is vidence that this blue substance is produced from, and at the expense f, the diurnal chromatophore-pigments. The blue substance passes rom the chromatophore-centres, persists for a time in the body, and ltimately disappears.

C. *Morphology.*

7. The chromatophore-system of Mysidean Schizopods is built upon common plan, of which the various genera and species present everally a constant modification. This system we call the primary hromatophore-system. To it the colour-pattern is due.

8. Decapod Crustacea possesses a primary and a secondary system f chromatophores. The primary system appears in the embryo, is ompleted in the "*Mysis*-stage," and persists throughout life, but takes o part in colour-pattern.

The secondary system arises in an early stage of development, ncreases in extent throughout life, and produces the colour-patterns of he adolescent and adult.

9. The chromatophores of the primary system are profusely ranched, few in numbers, segmentally arranged and centralised;

14. The adult colour-pattern of *Hippolyte cranchii* is constant, but develops indirectly. The adolescent possesses a special colour-pattern developed in large measure in relation with the primary system of the zoea. Both persist though concealed by the independently developed adult pattern.

15. In *Hippolyte varians*, several adult colour-patterns occur. They develop indirectly. The primary system is the same in all.

In the adolescent, three distinct colour-patterns arise:—"barred," "liner," and "monochrome."

These may persist, becoming barred, liner, or monochrome adults.

Or either "barred" or "liner" may, by developing superficial or deep chromatophores, become a monochrome.

Or, by localised superficial developments either "barred" or "liner" may give rise to a "blotched" adult colour-form, under which the adolescent pattern is hidden.

The primary system is inherited: the adolescent colour-patterns are possibly inherited; but inheritance is immaterial since the final goal is reached by any adolescent road; that is, the adult colour-pattern of *Hippolyte varians*, is the result of environment.

"Observations on 'Flicker' in Binocular Vision."

By C. S. SHERRINGTON, M.A., M.D., F.R.S. (Thompson-Yates Laboratory of Physiology, University College, Liverpool).

Received July 30, 1902.

The connection between the physiological state and reactions of the two retinæ right and left is close in many respects; this is true particularly and peculiarly for their areas that are conjugate in binocular vision, that is, which receive corresponding images of objects perceived in the binocular field. The observations at basis of the following communication attempt to obtain further information regarding the nature of the tie between these retinal so-called "identical spots." A practical aim was to measure by the "flicker" method of photometry any difference of physiological luminosity existent between binocular and uniocular vision of a given illuminated object.

An object intermittently illuminated gives, if the frequency of intermission be sufficient, a perfectly steady sensation. The successive retino-cerebral reactions fuze into a continuous one as judged of by sensation. If the rapidity of intermission be less than the requisite, the sensation oscillates through lighter and darker phases. The transition from the oscillating to the steady sensation and *vice versâ* is sufficiently abrupt to form a transition point capable of fairly definite fixation in time. It has been used in this enquiry as an index for noting the influence of the physiological state of a spot in one retina upon that of the "identical" spot in the other retina. A second index taken has been the visual "brightness" of the perceived image.

For the observations desired, it was considered important that extinction and re-illumination of the image occur *pari passu* in the two eyes, *i.e.*, with like speed, along a like direction in the retinal surface, and at "identical spots." It seemed also important to maintain perceptual singleness of the object seen, thus placing the "identical spots" in the most favourable condition for their co-operative identity. For this reason a steady and considerable degree of "convergence" of the visual axes was made one condition of the observations. The degree of focal accommodation of the eyes was arranged to correspond with the amount of convergence. Variations in the aperture of the pupil were excluded by wearing small artificial pupils.

The illuminated object was arranged as follows. A small double sheet of thick "milk" glass was illuminated by a candle-shaped, single-loop, incandescent electric lamp, with frosted glass front. The lamp was fed at rather above its intended voltage by accumulators unused during the experiment for any other purpose than the lighting of the lamp. The lamp was set in the axis of a rotating cylinder. In this

The spindle of the revolving cylindrical screen carried a step-pulley. From this a cord ran to a step-pulley fixed on the spindle of a small electromotor. The speed of running of this motor was controlled by a set of coil resistances, which formed a coarse adjustment, and by a fluid resistance in a trough 1 metre long, with a sliding electrode; this formed a fine adjustment. The speed of rotation of the cylindrical screen was recorded by marking the completion of each revolution of the spindle carrying the screen by an electro-magnetic signal writing on a travelling blackened surface. On the same surface the time was recorded by a writing clock marking fifths of seconds.

The observations required an operator to manage speed of motor, registration of time and revolutions, &c., and an observer who, seated in a dark compartment, gave his attention to the watching of the illuminated discs.

The shutters of the rotating cylindrical screen could be arranged so that the illumination of a retinal spot in one eye could be varied in time and intensity synchronously or asynchronously with that of the conjugate spot of the other eye. Of various combinations examined, the following may be cited as outlining the evidence obtained. The discs may be referred to as disc A and disc B.

Experiment 1.—When the disc A represents right retinal stimulation alone (*i.e.*, without left), the conjugate spot of the left eye remaining dark; and when the disc B represents right retinal stimulation with coincident synchronous (*i.e.*, of synchronous phase) intermittent stimulation of the conjugate spot of the left retina; the frequency, duration, and intensity of the light periods being alike at all the holes.

Then, steady sensation is obtained from disc A at frequencies of intermission lower than those required for giving steady sensation from disc B.

At speeds sufficient to give steady sensation at both discs, the disc B does not obviously differ in brightness from the disc A.

Experiment 2.—When disc A represents as before uniocular stimulation only,

And when disc B represents intermittent right retinal stimulation, together with a stimulation yielding steady sensation from the conjugate field of left retina.

And when the steady sensation from left retina corresponds with an intensity of light stimulus half that of the intermittent stimulus employed for the right eye, then the rate of intermission required for obtaining steady sensation from disc A is higher than that required for obtaining it from disc B.

At speeds sufficient to give steady sensation for both discs, the disc B does not obviously differ in brightness from the disc A.

Experiment 3.—The disc A representing uniocular vision as before.

When disc B represents intermittent right retinal stimulation with, same way as in 2, continuous steady sensation from the conjugate ft retinal spot,

And when the intensity of that continuous sensation from left tina corresponds with an intensity of light stimulus more than half at of each stimulus employed in repetition for the conjugate of the ght eye,

Then the rate of intermission required for obtaining steady sensa- on from disc A is higher than that required for obtaining it from the sc B.

Of the two discs, seen under speeds sufficient to give steady sensation om both, disc B appears the brighter.

Experiment 4.—The disc A representing uniocular vision as before.

When the disc B represents intermittent right retinal stimulation, gether with, in the same way as in 2 and 3, steady sensation from e conjugate left retinal spot,

And when the intensity of that steady sensation from left retina rresponds with that due to a light stimulus of less than half the in- nsity of each member of the series of repeated stimuli employed for e conjugate spot of the right eye,

Then the rate of intermission required for obtaining steady sensa- on from disc A is higher than that required for obtaining it from the tion from disc B does not appreciably differ from that required to obtain it from disc A.

Of the two discs, at speed sufficient to yield steady sensation, disc B does not obviously differ in brightness from disc A.

Experiment 7.—When disc A represents the binocular combination described for disk B, Experiment 1, and the disk B is as described in Experiment 6, the rate of intermission required to obtain steady sensation from A is higher than that required for obtaining it from B, but at speeds sufficient to yield steady sensation from both discs the two discs appear to be of equal brightness.

Experiment 8.—With disc A, as in Experiment 7, and disc B, as described in Experiment 6, the frequency of repetition of stimulus required to yield steady sensation from disc A is slightly higher than that required to obtain it from disc B, but at speeds sufficient to yield steady sensation from both discs the two discs appear of equal brightness.

Experiment 9.—With disc A, representing the binocular combination described for disc B in Experiment 5, and with disc B, as described in Experiment 6, the frequence of repetition of stimulus required to yield steady sensation at disc A is slightly lower than that required to yield it at disc B; but at speeds sufficient to yield steady sensation from both discs, the two discs appear of equal brightness.

The observations show (i) that Talbot's law, unimpeachable (over a wide range of ordinary luminosities) for the single eye, is not applicable to combined binocular vision, that is, that if the two eyes functioning together in binocular vision are considered as functionally to combine to a single organ, Talbot's law does not hold good for that organ as it does for the single eye, right or left. (ii) That increase of luminosity of an intermitting image does not always necessitate increase of rate of frequency to extinguish its flicker; and conversely, They also show that the "Fechner paradox" regarding binocular luminosity makes itself apparent under "flicker" examination as well as under "brightness" estimation.

It seems that the physiological sum of two luminosities perceived through conjugate retinal areas is of a value intermediate between the individual values of the two component luminosities.

Among experimental difficulties incident to the experiments may be mentioned the increased perception of flicker under paracentral as compared with central locus of stimulus on the retina, as noted by Exner and by Charpentier. Interesting experimental difficulties were also occasioned by the reciprocal and often antagonistic influences exerted by one retina upon another in ways studied and described recently by Dr. W. McDougall.*

It was further observed that binocular colour mixture did not seem be either rendered easier or impeded when the components were pplied by alternating right and left stimuli as compared with the ethod of applying them by coincident right and left stimuli.

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* 'Mind,' 1901.

On the Influence of the Prolonged Action of the Temperature of Liquid Air on Micro-organisms, and on the Effect of Mechanical Trituration at the Temperature of Liquid Air on Photogenic Bacteria."

By Allan Macfadyen, M.D. Communicated by Professor James Dewar, F.R.S.

Received August 2, 1902.

In previous communications it was shown that an exposure for enty hours and for a period of seven days to the temperature of quid air (about − 190° C.) had no effect on the vitality of microganisms, whilst an exposure of ten hours to a temperature as low as at of liquid hydrogen (about − 252° C.) was likewise without an ppreciable effect.*

Further experiments have since been made in which the influence of e prolonged action of the temperature of liquid air on organisms

The ordinary manifestations of life cease at zero, but at − 190° C. we have reason to suppose that intracellular metabolism must in addition practically cease—as a result of the withdrawal of two of its cardinal physical conditions, viz., heat and moisture. It is difficult to form a conception of living matter under this new condition, which is neither life nor death, or to select a term which will accurately describe it.

In previous experiments it was found that the photogenic bacteria preserved their normal luminous properties after exposure to the temperature of liquid air. On re-thawing, a rapid renewal of the photogenic properties of the cells occurred. The light is apparently produced by a chemical process of intracellular oxidation. The feasibility of triturating micro-organisms at the temperature of liquid air has now been experimentally established in the case of the typhoid bacillus and other bacteria.* The effect of such mechanical trituration at the temperature of liquid air on the luminous properties of the photogenic bacteria has now been tested. The experiments have shown that the effect of such a trituration is to abolish the luminosity of the cells in question.

This points to the luminosity being essentially a function of the living cell, and dependent for its production on the intact organisation of the cell.

I am indebted to Professor Dewar for valuable suggestions, and to Mr. Sydney Rowland and Mr. J. E. Barnard for their assistance in the experiments, which were carried out at the Jenner Institute of Preventive Medicine.

"An Intracellular Toxin of the Typhoid Bacillus." By Allan Macfadyen, M.D., and Sydney Rowland, M.A. Communicated by Lord Lister, F.R.S. Received August 14, 1902.

The existence of a specific toxin produced by the typhoid bacillus has hitherto not been demonstrated, although it has been assumed by analogy with other organisms and by reasoning from the clinical course of the disease.

Such a poison must either be intracellular or extracellular.

That it does not exist in filtered cultures of the organism is the common experience of bacteriologists. Its absence from such cultures might be due, however, to unsuitability of the soil used for growing the organism.

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* "The Intracellular Constituents of the Typhoid Bacillus," Allan Macfadyen and Sydney Rowland, 'Centralblatt f. Bakteriologie,' vol. 30, 1901, No. 20.

Accordingly the first step in the search for the body in question nsisted in substituting for the usual broth and peptone media, lture fluids approaching more nearly in constitution the natural dy soils which clinically support the growth of the bacillus. For is purpose, the organism was grown on the actual intracellular ices of the following organs and tissues obtained in a fresh condion from the ox or calf:—

Intestinal mucous membrane, mesenteric lymphatic glands and leen. In each case the intracellular juice was brought to the requite degree of alkalinity and used as a culture soil under the following nditions:—

1. Aerobically. 2. Anaerobically. 3. With addition of normal uman serum. 4. After heating to 55° C. for 20 minutes.

After from 4 to 6 weeks' growth the organisms were filtered off and e filtrate tested for toxicity in guinea-pigs. With the possible ception of one spleen juice, none of the fluids thus obtained exibited any acute toxic power. It thus became necessary to search ithin the typhoid organism itself for the missing toxin. For this urpose the organisms were grown on ordinary beef broth agar, and ter careful washing with distilled water were disintegrated in a echanical contrivance at the temperature of liquid air (−180° C.)

"The Fracture of Metals under repeated Alternations of Stress."

By J. A. Ewing, LL.D., F.R.S., Professor of Mechanism and Applied Mechanics in the University of Cambridge, and J. C. W. Humfrey, B.A., St. John's College, Cambridge, 1851 Exhibition Research Scholar, University College, Liverpool.

Received August 11, 1902.

(Abstract.)

The paper describes an investigation by means of the microscope of the process by which iron becomes "fatigued" and breaks down when subjected to repeated reversals of stress, as in Wöhler's experiments. It is shown that although the greatest stress is much within the limit of elasticity (as determined by the proportionality of strain to stress in an ordinary tensile test), it produces rupture after many reversals. The first visible effect is the production of slip-bands here and there on individual crystals. These gradually become more numerous: they also become accentuated and broadened and their edges turn rough and burred, apparently as a result of grinding of one surface on the other over the plane in which slip has occurred. At a later stage certain of the slip-bands develop into cracks, whose existence can be demonstrated by repolishing the specimen, when the slip bands which have not opened into cracks are obliterated, but the cracks remain visible as actual fissures. As the process of reversals goes on, the cracks spread from crystal to crystal, and fracture ensues. In the particular material dealt with, Swedish iron, having an elastic limit in tension of about 13 tons per square inch and a breaking strength of 23·6 tons per square inch, it was found that a stress not exceeding 9 tons per square inch, when reversed some millions of times, was sufficient to develop cracks and to bring about the fracture of the piece. Stresses of 8 and even 7 tons per square inch were found to develop slip bands which would probably turn into cracks under a sufficient number of reversals. The paper is illustrated by micro-photographs taken at various stages of the destructive process.

On Changes in Elastic Properties produced by the sudden Cooling or 'Quenching' of Metals."

By JAMES MUIR, B.A., D.Sc., late 1851 Exhibition Science Research Scholar. Communicated by Professor EWING, F.R.S. Received August 11, 1902.

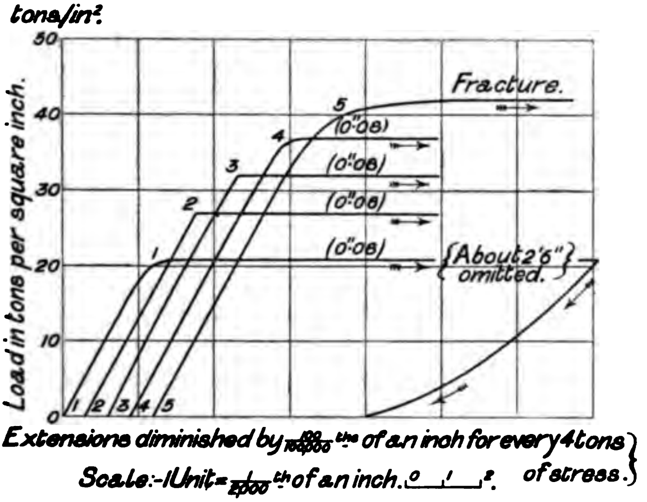
(Being part of a Thesis submitted for the degree of Doctor of Science, Glasgow University.)

It is well known that when steel is quenched from a red heat, its
astic properties suffer a profound change, the material becoming
xtremely hard and brittle. It is also known that quenched steel,
hen tested under tension, exhibits no distinct yield-point, Hooke's
w is departed from quite gradually until abrupt fracture occurs at a
igh stress. The effect produced on copper by quenching has been
nsidered, at least in some respects, the reverse of that produced in
eel. The experiments to be described in this paper, however, show
at with mild steel, soft iron, copper, zinc, aluminium, brass, and so
obably with all metals, quenching from high temperatures produces
fects which are analogous to one another; in all cases there is a

fully described in a paper by the present author "On the Recovery of Iron from Overstrain."* The amount by which the curves have been sheared back is marked at the foot of each diagram. Thus, in Diagram No. 1, $\frac{100}{1000000}$ths of an inch have been deducted from the extension of the 4-inch length for every 4 tons of stress. For example, the extensometer readings for stresses of 4, 8, and 12 tons per square inch were 120, 240, and 360 respectively; the numbers actually plotted were 20, 40, and 60. The origin for the measurement of extensions has been displaced for the various curves of each diagram in order to avoid a confusion of the curves.

Diagram No. 1 shows the elastic properties of an annealed specimen of mild steel. The specimen was subjected to a series of tension tests, the load in each test being carried just to a yield-point. Recovery from the overstrains produced by the passing of the successive yield-points was effected by heating the specimen to temperatures of from

Diagram No. 1.—(Mild steel-annealed.)



Diameter of specimen = 0"·331.
Length under test = 4"·00.
Fracture occurred at 40 tons per square inch original area. Extension (including all yield-points) = 0"·38 on 4 inches.

200° to 250° C. The specimen broke at the fifth yield-point, the breaking stress being 42 tons per square inch, or about 40 tons per square inch taking the original area of the specimen. After the passing of each yield-point the diameter of the specimen was of course ghtly reduced ; this was allowed for in the succeeding loadings, the ad being always applied in tons per square inch of actual section.

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* 'Phil. Trans.,' A, vol. 193, 1899.

Diagram No. 2 shows the elastic properties of the same steel after it ad been heated to 500°, to 650°, and to 700° C., and quenched in ater at about 15° C. Each of the specimens employed was broken y a single continuous loading. Curves A and B show that quenching om 500° and from 650° C. had little effect on the elastic properties of e steel. The specimen from which Curve B was obtained had been ore thoroughly annealed before quenching than Specimen A, and is may account for the lower breaking load and greater ultimate tension obtained with Specimen B, although all the specimens nployed were primarily annealed. Curve C shows that a marked ange was produced in elastic properties by quenching from 700° C.

DIAGRAM No. 2.—(Mild steel-quenched.)

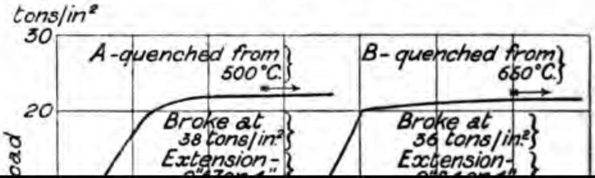
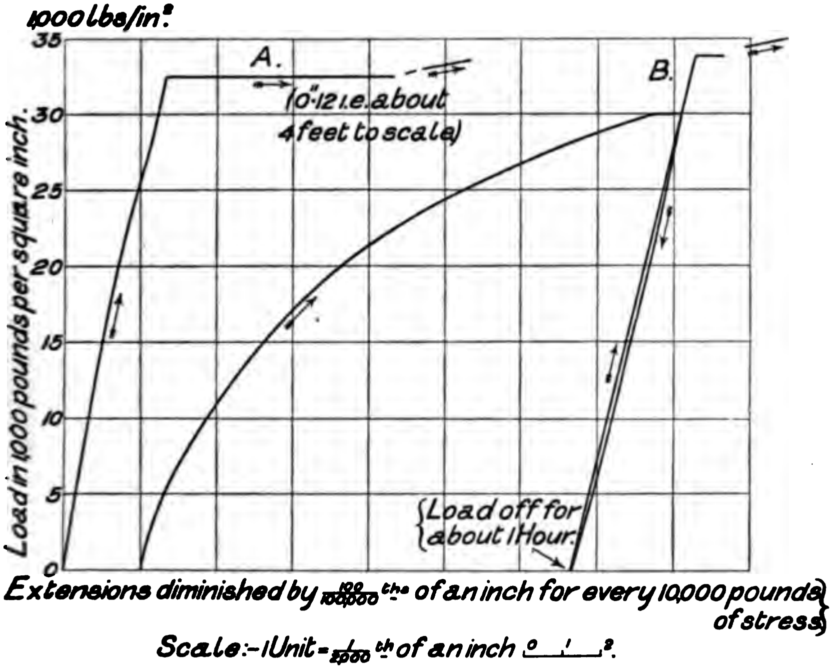


Diagram No. 3 shows the effect produced on the elastic properties of Lowmoor iron by quenching from 700° C. Curve A was obtained from an annealed specimen of the material. A very clearly defined yield-point is shown at the stress of 33,000 lbs. per square inch. After the yielding at the yield-point had spread throughout the specimen, the load was steadily increased until fracture occurred at the stress of 50,300 lbs. per square inch. The extension produced was 0″·95 on 4 inches, neglecting all the local extension which occurred at the point of fracture, or 1″·23 including the local extension.

Curve B, which was obtained from a specimen which had been quenched from 700° C., clearly shows the loss of elasticity produced by quenching. A curious recovery effect was noticed in this test. The load was applied until a stress of 30,000 lbs. was attained, and

DIAGRAM No. 3.—(Lowmoor Iron.)



Diameter of specimens A and B = 0″·44.
Length under test = 4″·00.

Specimen A.—Annealed at 750° C. Broke at 50,300 lbs. per square inch.
Extension 0″·95 omitting, or 1″·23 including, local extension.

Specimen B.—Heated to 740° C., slowly cooled to 700° C., and then quenched in cold water. Broke at 61,300 lbs. per square inch.
Extension 0″·61 omitting, or 0″·88 including, local extension.

was then removed. The contraction which occurred on the removal of the load was almost perfectly elastic. Had the load been immediately replaced, the material would have shown perfect elasticity up to the stress of 30,000 lbs., but immediately the load was increased beyond this amount larger yielding would have occurred, and a smooth continuation of Curve B would have been obtained. The specimen was, however, allowed to rest for about an hour before the load was replaced and increased. This rest proved to have a comparatively large effect, the material showing very perfect elasticity up to the stress of 34,000 lbs. per square inch. At this stress a partial yield-point was exhibited (represented by about 6 units of extension on the diagram), and on increasing the load gradual extension was produced, until ultimately fracture occurred at the high stress of 61,300 lbs. per square inch. The ultimate extension is marked at the foot of the diagram, and was less than that obtained with annealed material.

It may be recorded that another specimen of this Lowmoor iron was quenched from 700° C., but, before testing, this specimen was re-heated to about 200° C. in order to see if any appreciable return to the elastic condition illustrated by Curve A, Diagram 3, would be obtained. The behaviour of the specimen was more nearly elastic for low loads than is shown by Curve B, but all the main features of Curve B were corroborated; gradual departure from Hooke's law was obtained until fracture occurred at 61,500 lbs. per square inch, the extension being 0″·54 on 4 inches omitting, or 0″·9 including, local extension.

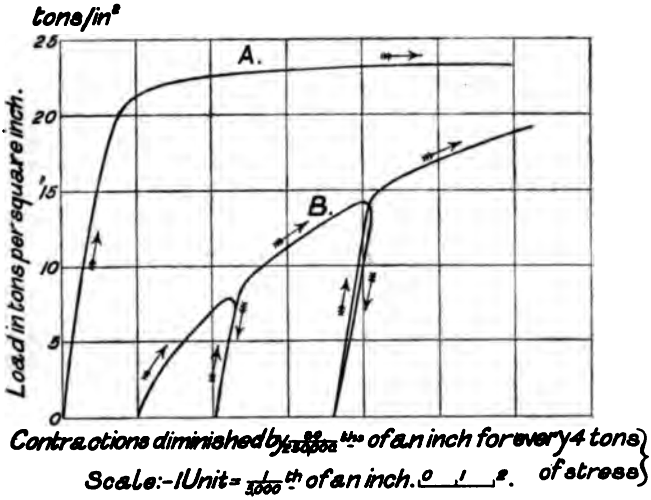
Before leaving the consideration of iron and steel, the effect produced by quenching iron, as illustrated by compression tests, may next be considered.

Diagram No. 4 shows by a comparison of two compression curves the change in elastic properties produced by quenching mild steel from a red heat. Specimen A, was a short annealed block of mild or semi-mild steel; the diameter of the specimen was 1″·156 and its length 1⅞ inches. The compression instrument employed enabled the contraction on a length of 1¼ inches to be measured to the $\frac{1}{25000}$ of an inch. Curve A shows that the annealed material was elastic up to the stress of 21 tons, but 23 tons per square inch had to be applied before really large yielding occurred. A tension test of this material showed a well-defined yield-point at 22½ tons per square inch.

Specimen B was exactly similar to Specimen A, but the material in this case instead of being annealed was heated to redness and quenched in cold water. Curve B shows the marked loss of electricity produced by the quenching. The rounding of Curve B at the two points where the load was removed is probably to be accounted for by experimental errors of the nature of back-lash in the testing machine or compression instrument.

Diagram No. 5 illustrates the results obtained by experiments with copper rods. Curve A of that diagram was plotted from a tensile test made with the material in the condition as supplied. Curve B shows the elastic properties of the material after it had been heated to 630° C., and allowed to cool slowly, while Curves C_1, C_2, and C_3 show the effect produced by quenching the copper from 500° C., from 550° C., and finally from 600° C. Specimen B showed more perfect elastic behaviour for low loads than Specimen A, but large extension is shown by Curve B to have occurred earlier with the annealed material. Specimen C was first heated to 500° C., and quenched in cold water.

DIAGRAM No. 4.—(Mild steel under compression.)

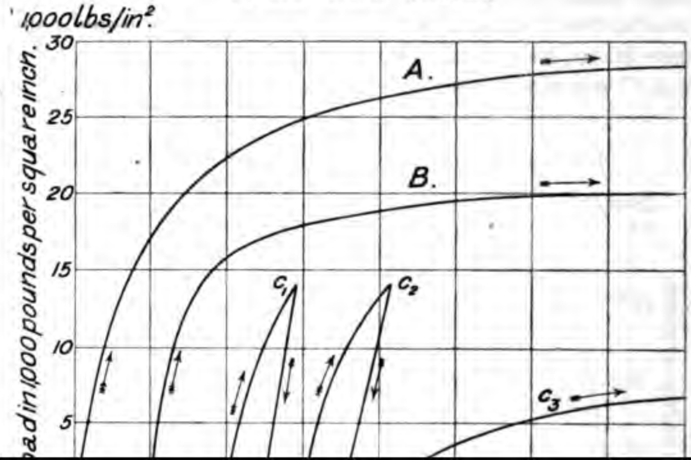


Specimen A.—Annealed.
" B.—Quenched from a red heat.

Diameter of specimens = 1·16 inches.
Total length = 1⅞ inches.
Contraction measured on 1¼ inches.

Curve C_1 was then obtained by applying and removing a load of 15,000 lbs. per square inch. The specimen was next quenched from 550° C., and Curve C_2 shows the slightly greater loss of elasticity which was thus produced. Curve C_3 shows the large effect caused by quenching the Specimen from 600° C. The breaking stresses obtained with the three specimens were 35,300, 33,500, and 32,300 lbs. per square inch of original area. These stresses were equivalent to 41,600, 40,800, and 44,200 lbs. per square inch, when allowance was made for the diminutions in area due to the large extensions of the specimens before fracture. These corrections were made by lculating the reduced areas from the extensions obtained (omitting e local extensions at points of fracture) and neglecting the small anges in density which are known to be produced by stretching. opper thus resembles iron and steel in having its breaking stress

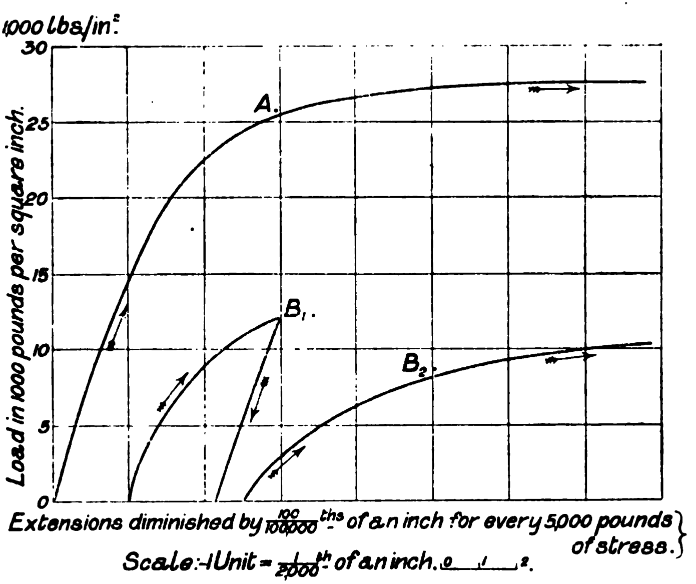
DIAGRAM No. 5.—(Copper.)



the local extensions at the points of fracture, or 1″·04, 1″·25, and 1″·98 respectively including the local extensions.

Diagrams Nos. 6 *and* 7 may now be given without comment. They illustrate tests made with brass and aluminium, and it is shown in both cases that there is a loss of elasticity produced by quenching. When the quenched material has been once loaded it is brought approximately into the elastic condition, so that from a removal and reapplication of load a straight stress-strain curve is obtained.

Diagram No. 6.—(Brass.)



Diameter of specimens = 0″·44. Length under test = 4″·000.

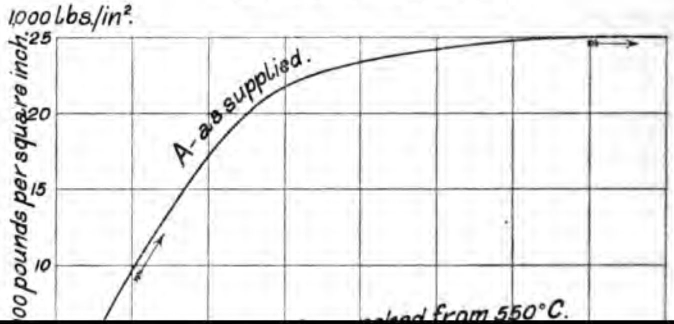
Specimen A.—Brass as supplied. Broke 4 times in machine grips at about 48,500 lbs. per square inch. Extension from 0″·87 on 4 inches after the first break to 1″·68 after the fourth.

Specimen B.—Brass quenched from 600° C. (B_1) and from 700° (B_2). Broke at 42,500 lbs. per square inch after two breaks in the machine grips at slightly lower stresses. Extension, 1″·33 on 4 inches.

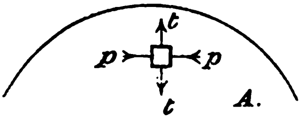
Three specimens of zinc, of diameter 0″·40, were also tested. The first—in the condition as supplied—broke at 21,000 lbs. per square inch, the ultimate extension being 2″·04 on 4 inches. There was great local extension, the specimen being drawn to a very narrow neck before fracture. The second specimen was quenched from 350° C. It yielded rather more for the lower loads than the first specimen, and broke at 20,500 lbs. per square inch with an extension of 0″·41 on 4 inches. The fracture was quite abrupt, so that there was little or no cal extension. The third specimen was heated to 350° C., and llowed to cool in the air. The behaviour of this specimen was very imilar to that of the quenched specimen. Rather less yielding was btained at low stresses, fracture occurred at 21,400 lbs. per square nch, the ultimate extension was 0″·68 on 4 inches. The fracture was brupt, so that there was practically no local extension.

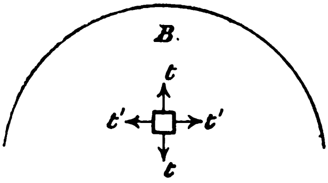
Two specimens of cast tin were also tested, but owing to the low elting point of tin quenching from 200° C. could only be tried. The

Diagram No. 7.—(Aluminium.)



interior will then contract, and exert a radial pull on the outside solidified layer. This will put the material into a state of circumferential compression. If the tangential direction be called the direction of X, the radial direction that of Y, so that the rod considered lies along the axis of Z, then the material in the outside layer of the quenched rod of metal is subjected to a compressional stress in the X direction. If a layer of material be considered at some distance from the outside it will be found to be subjected not only to a compression in the X direction, but also to a tension in the Y direction. For the outside solidified layers are able to resist to some extent the radial pull due to contraction. A particle of material at a point such as A will thus be subjected to stresses p and t in the manner illustrated in the sketch. Going nearer the centre of the bar, the pull due to contraction of the hot material may be more than balanced by the outward radial pull due to the solidified material which has settled down under radial tension, so there may be a resultant outward pull all round the layer considered, and a particle such as B will be subjected to a circumferential pull, t', as well as a radial pull, t. There will be, of course, a gradual transition from material in the one condition to material in the other.





Further, the stresses induced by sudden cooling will probably be severe enough to overstrain many layers of material, and, except in the case of portions which have been overstrained when quite cool, recovery from overstrain will be effected, so that the material will be left in an elastic condition, hardened as regards the stresses in question, and not in the semi-plastic state typical of material which has been recently subjected to overstrain.

Now it is well known that when metals are deformed they alter very little in volume, almost the whole strain is one due to change of shape. It is only necessary then to consider the shear stresses applied by the systems of stresses illustrated above at A and B. A pull (t or t') is equivalent to a hydrostatic tension ($\frac{1}{3}t$ or $\frac{1}{3}t'$) and two shear stresses in definite directions; a push (p) gives rise to a hydrostatic pressure ($\frac{1}{3}p$) and two shear stresses. A circumferential pressure (p case A) gives rise to the following two shears:—

1_{A}, giving contraction in direction of X and extension in direction of Y,
2_{A} ,, ,, ,, X ,, ,, Z.

A radial tension (*t* cases A and B) gives rise to the following two ears :—

1_{AB}, giving extension along Y and contraction along Z,
2_{AB} ,, ,, ,, Y ,, ,, X.

A circumferential tension (*t'* case B) gives rise to the following two ears :—

1_{B}, giving extension along X and contraction along Y,
2_{B} ,, ,, ,, X ,, ,, Z.

It is necessary then to consider what effect these shear stresses, duced by quenching, have on the behaviour of a bar subjected to a nsion (T) or a pressure (P) in the direction of the Z axis.

A pull, T, in direction of the Z axis gives rise to the following two ears :—

1_{T} producing extension along Z and contraction along X,
2_{T} ,, ,, ,, Z ,, ,, ,, Y.

A push, P, along the Z axis produces the following two shears :—

1_{P} giving contraction along Z and extension along X, take place, and perhaps also a redistribution of the stress due to the applied load. This alteration in the distribution of the internal stresses must be such as to cause the surrounding strong layers to stretch elastically as far as the weak material has been permanently stretched. The alteration in the internal stresses will remain after the applied load is removed, as the material which has been permanently deformed will be unable to relieve the stronger material. The apparent permanent set which is shown with quenched material after the removal of applied load, may thus be due to the real permanent extension only of the weak layers, and to the elastic extension of the strong layers produced by the new distribution of internal stresses. This explanation, however, does not suffice, at least in the case of iron and steel, to explain the behaviour of a quenched rod under applied stress, for Diagrams 2 and 3 show that such a rod may be stretched further than is compatible with elastic extension—even supposing some of the iron to have been overstrained to the maximum in the most favourable direction, without stretching nearly far enough for the yield-point of the iron to have been passed. Hence in the case of iron and steel recourse must be had to the explanations which simply attribute the observed effects to the formation of allotropic modifications of the metal or to the changes caused by the transition of the carbon—always present—from one condition to another.

In conclusion, it may be recorded that pieces of the iron and steel specimens used in this research were polished, etched, and examined under the microscope. In the case of the steel specimens the change from the ferrite and pearlite structure shown with the annealed material to the martensite structure shown with the quenched steel was very striking. But in the case of the Lowmoor iron no difference was detected by the microscope in the structures of the annealed and of the quenched specimens, although, as shown by Diagram 3, the elastic properties in the two conditions were vastly different.

"Harmonic Tidal Constants for certain Australian and Chinese Ports." By THOMAS WRIGHT, of the Nautical Almanac Office. Communicated by Professor G. H. DARWIN, F.R.S.

Received August 1, 1902.

Ballina (New South Wales), Princess Royal Harbour (King George's Sound), Newcastle (New South Wales), Brisbane (Queensland), and Sydney (New South Wales).

The tidal observations made at these five ports have been reduced by the aid of certain sums placed at my service by the Government Grant Committee of the Royal Society, and I am indebted to Professor

G. H. Darwin for the loan of the apparatus he devised to facilitate the summation of hourly tidal heights, and to the Hydrographer, Admiral Sir W. J. L. Wharton, who supplied me with the observations. The whole of the observations were reduced by the methods devised by Professor G. H. Darwin.*

The observations made at the three first-mentioned ports were derived from copies of continuous diagrams made by automatic tide gauges; those at Brisbane and Sydney were times and heights of high and low water. The observations in every case extended over a period of about 1 year, and were almost complete. The breaks in the continuity of the observations were so short that approximate values could be easily inserted by interpolation with very small risk of error.

From the automatic records at Ballina, Princess Royal Harbour, and Newcastle the hourly heights were read off to the nearest one-tenth of a foot. The range of the tide at these ports is small, and an attempt was made to use a smaller unit, one-twentieth of a foot. An experiment with one month's observations showed, however, that the hourly and daily sums for the month differed very slightly, whether the readings were taken to the nearest one-twentieth foot or to the nearest one-tenth foot only. Besides, when the diagrams for two consecutive days were placed end to end there frequently appeared to be agree, it seems hardly necessary to check the entering on the strips. In all other respects Professor Darwin's methods were followed exactly.

As already stated, the observations made at Brisbane and Sydney were the times and heights of high and low water. They were reduced by Professor Darwin's method. The observations were split up into four groups, each covering about one-fourth of a year. Each of these groups was separately reduced, and means of the four values of κ and H for each tide was taken as the final constant. These separate values form a check on the work independently of the systematic method of verification which was adopted in each stage of the computation. In the case of the more important tides, the agreement among the four values is very close. In the case of some of the smaller tides the differences are somewhat greater, but not great enough to make any serious difference in predictions based upon the constants.

The constants for the five ports are given, with others, below. The small value of H (0·159 foot) for M_2 at Princess Royal Harbour gave rise to the suspicion that there was some error in the work. This value is, however, borne out by the value of H for M_2 at Batavia (Java) given in the American Tide Table for 1900. Batavia is there quoted as a "Standard Port for Reference" for King George's Sound, and the height there given is 0·154 foot, or only 0·005 foot different from that obtained for Princess Royal Harbour. Careful examination of the work showed, too, that the value given below is correct.

Hong Kong, Swatow, Whampoa, Cooktown, and Cairn's Harbour.

The constants for these ports have been deduced at various times during the past few years. Except in the case of some of the Hong Kong tides, constants for these ports have not yet been published, and the present opportunity is taken to include them with the others.

The observations made at Hong Kong, Swatow, and Whampoa were from records by automatic gauges; those at Cooktown and Cairn's Harbour were observations of times and heights of high and low water. They were reduced by the same methods as the observations at the other five ports. The observations for the Chinese ports were kindly supplied by the Chinese Customs authorities; those for Cooktown and Cairn's Harbour by the Hydrographer. There seemed reason to suppose that the observations at Whampoa had not been very good, and the results for the tides S_6, R, M_1, M_8, L, μ, 2SM, J, Mf, and M_m seem to be so uncertain that I have thought it best to omit them from the Table of Values. For a like reason the L tide is omitted from the results for Cooktown and Cairn's Harbour.

tow. 7–8.	Whampoa 1894–5.	Brisbane Bar. 1865–6.	Sydney. 1888.	Cooktown. 1890.	Cairn's Harbour. 1892–3.
23′ N. 39′ E.	23° 5′ N. 113° 26′ E.	27° 31′ S. 153° 0′ E.	33° 52′ S. 151° 12′ E.	15° 27′ S. 145° 15′ E.	16° 55′ S. 145° 47′ E.
386	5·722	7·120	2·449	16·624	4·476
065 56	0·119 118	— —	— —	— —	— —
316 6	0·666 64	0·579 315	0·375 268	0·788 258	1·120 245
025 16	0·003 16	— —	— —	— —	— —
001 6	* *	— —	— —	— —	— —
019 8	* *	— —	— —	— —	— —
347 3	2·184 32	2·201 290	1·636 254	1·873 282	1·958 282
038 41	0·100 190	— —	— —	— —	— —
228 54	0·160 313	— —	— —	— —	— —

M_6 { H =	0·002	0·018	0·025	0·017	0·053	*	—	—	—	—
κ =	227	74	133	168	172	*	—	—	—	—
O { H =	0·417	0·289	0·310	0·949	0·765	0·815	0·325	0·337	0·290	0·407
κ =	312	88	128	245	254	310	139	86	113	166
K_1 { H =	0·623	0·513	0·454	1·191	0·941	1·068	0·592	0·419	0·287	0·872
κ =	330	120	155	294	292	354	176	129	171	190
K_2 { H =	0·074	0·127	0·072	0·184	0·119	0·288	0·158	0·102	0·215	0·305
κ =	338	254	273	272	73	64	315	268	258	245
P { H =	0·172	0·153	0·139	0·388	0·269	0·351	0·196	0·139	0·096	0·290
κ =	332	116	149	292	285	15	176	129	171	190
J { H =	0·037	0·035	0·031	0·032	0·025	*	—	—	—	—
κ =	356	141	184	307	299	*	—	—	—	—
Q { H =	0·091	0·072	0·077	0·172	0·139	0·116	—	—	—	—
κ =	291	165	103	228	242	326	—	—	—	—
L { H =	0·039	0·069	0·047	0·026	0·080	*	0·180	0·065	*	*
κ =	349	238	286	284	34	*	258	237	*	*
N { H =	0·067	0·351	0·202	0·301	0·237	0·380	0·458	0·324	0·447	0·658
κ =	17	235	254	255	358	16	288	250	239	269
ν { H =	0·020	0·123	0·018	0·012	0·074	0·047	—	—	—	—
κ =	33	245	169	133	327	352	—	—	—	—
μ { H =	0·019	0·061	0·026	0·072	0·070	*	—	—	—	—
κ =	16	226	335	237	176	*	—	—	—	—
R { H =	0·043	0·011	− 0·006	0·007	0·043	*	—	—	—	—
κ =	192	213	347	60	180	*	—	—	—	—

tow.	Whampoa.	Brisbane Bar.	Sydney.	Cooktown.	Cairn's Harbour.
7–8.	1894–5.	1865–6.	1888.	1830.	1892–3.
23′ N. 39′ E.	23° 5′ N. 113° 26′ E.	27° 31′ S. 153° 0′ E.	33° 52′ S. 151° 12′ E.	15° 27′ S. 145° 15′ E.	16° 55′ S. 145° 47′ E.
·015	0·036	—	—	—	—
82	23	—	—	—	—
·103	0·144	—	—	—	—
00	359	—	—	—	—
·025	*	—	—	—	—
91	*	—	—	—	—
·050	*	—	—	—	—
98	*	—	—	—	—
·069	*	—	—	—	—
20	*	—	—	—	—
·074	0·270	—	—	—	—
0	59	—	—	—	—
·467	0·484	0·109	0·093	0·346	0·202
70	171	8	16	320	9
·258	0·135	0·005	0·008	0·051	0·050
6	92	156	97	36	157

"On some Definite Integrals and a New Method of reducing a Function of Spherical Co-ordinates to a Series of Spherical Harmonics."

By ARTHUR SCHUSTER, F.R.S. Received May 30,—Read June 5, 1902.

(Abstract.)

The expansion of a function $f(\theta)$ of an angle θ varying between 0 and π in terms of a series proceeding by the sines of the multiples of θ depends on the fundamental theorem,

$$\int_0^\pi \sin p\theta \sin q\theta \, d\theta = 0,$$

where p and q are integer numbers not equal to each other. Similarly if P_n denotes the zonal harmonic of degree n, $\mu = \cos\theta$, and

$$Q_n^\sigma = \sin^\sigma\theta \frac{d^\sigma P_n}{d\mu^\sigma},$$

the expansion of a function of θ in terms of a series of the functions Q_n^σ depends on the corresponding theorem,

$$\int_{-1}^{+1} Q_n^\sigma Q_i^\sigma d\mu = 0,$$

where i and n are two integer numbers not equal to each other. In many practical applications a continuous function is given by means of its numerical values at certain points, *e.g.*, for equidistant values of θ.

Such cases present no difficulty when Fourier's analysis is to be employed, because there is in that case a summation theorem exactly corresponding to the above integration theorem. If θ be replaced by $\rho\pi/n$, where ρ takes successively the values 1, 2, 3 . . . , the equation

$$\sum_{\rho=0}^{\rho=n-1} \sin(\rho p\pi/n)\sin(\rho q\pi/n) = 0$$

will hold true. This allows us to determine the coefficients in the case of problems in which discontinuous values of the function at equidistant points are known (*e.g.*, hourly readings of temperature or barometric pressure). If we assume that all Fourier coefficients beyond the nth vanish, n equations are obtained, each of which only contains one of the unknown quantities.

If it is desired to expand a function in terms of cosines, a slight odification must be introduced, the summation theorem in that case eing

$$\tfrac{1}{2}+\tfrac{1}{2}\cos p\pi \cos q\pi + \sum_{\rho=1}^{\rho=n-1} \cos(\rho p\pi/n)\cos(\rho q\pi/n) = 0,$$

he first and second terms representing half the value of the product or $\rho = 0$ and $\rho = n$ respectively.

There is no corresponding summation theorem in the case of the unctions Q_n^σ, and the application of the method of least squares leads o a series of normal equations, each of which contains *all* the other oefficients. This has been one of the great practical difficulties in btaining an expression for the series of spherical harmonics for the arth's magnetic potential.

F. E. Neumann has tried to overcome the difficulty by calculating oefficients $a_1, a_2 \ldots a_q$ in such a way that

$$\sum_{\rho=1}^{\rho=q} a_\rho \, Q_n^\sigma(\mu_\rho)\, Q_i^\sigma(\mu_\rho) = 0.$$

Here $\mu_1, \mu_2 \ldots \mu_q$ are the quantities for which the values of the unction to be represented are known. Neumann's process is equivalent to attaching weights proportional to a_ρ to the different obser-

When it is therefore desired to retain only terms as far as the nth degree, the Fourier coefficients need only be calculated as far as $p = n+1$. The position of the earth's magnetic axis, *e.g.*, only depending on the terms of the first degree, is completely determined by the coefficients b_2 for $\sigma = 0$ and a_0, a_2 for $\sigma = 1$.

3. The symbolical representation of the results of this paper is much facilitated by the introduction of a separate symbol for the product of alternate factors, $n \,.\, n-2 \,.\, n-4 \ldots 1$, if n be odd, or $n \,.\, n-2 \ldots 2$ if n be odd. I propose to write $n\,!!$ for such products, and if a name be required for the product to call it the "alternate factorial" or the "double factorial." Full advantage of the new symbol is only gained by extending its meaning to negative values of n. Its complete definition may then be included in the equations

$$n\,!! = n(n-2)\,!!\,, \qquad 1\,!! = 1\,, \qquad 2\,!! = 2\,.$$

From this we may derive when n is negative and odd

$$n\,!! = (-1)^{\frac{n+1}{2}} \frac{1}{(-n-2)\,!!},$$

while for n negative and even, the factorial becomes infinitely large.

4. The calculation of the factors A_n^σ and B_n^σ depends on the values of the definite integrals

$$\int_{-1}^{+1} Q_n^\sigma \cos p\theta d\mu, \qquad \int_{-1}^{+1} Q_n^\sigma \sin p\theta d\mu,$$

and these may be made to depend on the values of the integrals

$$\int_{-1}^{+1} Q_n^\sigma \sin^\lambda\theta d\mu \qquad \text{and} \qquad \int_{-1}^{+1} \mu Q_n^\sigma \sin^\lambda\theta d\mu\,.$$

It is proved that

$$\int_{-1}^{+1} Q_n^\sigma \sin^\lambda\theta d\mu = c\frac{(n+\sigma-1)\,!!\,(\sigma+\lambda)\,!!\,(n-\lambda-2)\,!!}{(n-\sigma)\,!!\,(\sigma-\lambda-2)\,!!\,(n+\lambda+1)\,!!}, \quad \text{if } n-\sigma \text{ be even,}$$

$$= 0 \qquad \text{if } n-\sigma \text{ be odd,}$$

$$\int_{-1}^{+1} \mu Q_n^\sigma \sin^\lambda\theta d\mu = c\frac{(n+\sigma)\,!!\,(\sigma+\lambda)\,!!\,(n-\lambda-3)\,!!}{(n-\sigma-1)\,!!\,(\sigma-\lambda-2)\,!!\,(n+\lambda+2)\,!!}, \quad \text{if } n-\sigma \text{ be odd,}$$

$$= 0 \qquad \text{if } n-\sigma \text{ be even.}$$

The factor c is equal to 2 or to π according as $\sigma+\lambda$ is even or odd.

5. The integrals

$$\int_{-1}^{+1} Q_n^\sigma Q_i^\rho \, d\mu$$

e obtained in the form of a series having a finite number of rms.

6. To find

$$\int_{-1}^{+1} Q_n^{\sigma} \sin p\theta d\mu \quad \text{and} \quad \int_{-1}^{+1} Q_n^{\sigma} \cos p\theta d\mu,$$

e may either express Q_n^{σ} or the trigonometrical functions in terms of eries of powers of $\sin \theta$. The second alternative leads to results hich in general are more convenient.

If we put

$$C_0 = 1; \quad C_1 = p; \quad C_2 = p \,.\, \frac{p}{2}; \quad C_3 = p \,.\, \frac{p-1 \,.\, p+1}{1 \,.\, 2 \,.\, 3};$$

$$C_{\lambda} = \frac{p}{\lambda\,!} \frac{(p+\lambda-2)\,!\,!}{(p-\lambda)\,!\,!};$$

$$B_0 = 1; \quad B_1 = p; \quad B_2 = \frac{p-1 \,.\, p+1}{1 \,.\, 2}; \quad B_3 = \frac{p-2 \,.\, p \,.\, p+2}{1 \,.\, 2 \,.\, 3}$$

$$B_{\lambda} = \frac{1}{\lambda\,!} \frac{(p+\lambda-1)\,!\,!}{(p-\lambda-1)\,!\,!},$$

the longitude, the results of the investigation may be put into the form

$$g_n^\sigma = \sum_{p=0}^{p=n+1} a_p^\sigma r_n^\sigma, \text{ when } n \text{ is even, } \sigma \text{ odd, and } p \text{ odd.}$$

$$= \sum_{p=1}^{p=n+1} a_n^\sigma n_n^\sigma \quad \text{,,} \quad n \text{ ,, odd, } \sigma \text{ ,, } \quad \text{,, } p \text{ even.}$$

$$= \sum_{p=1}^{p=n+1} b_p^\sigma m_n^\sigma \quad \text{,,} \quad n \text{ ,, even, } \sigma \text{ even, ,, } p \text{ odd.}$$

$$= \sum_{p=2}^{p=n+1} b_p^\sigma s_n^\sigma \quad \text{,,} \quad n \text{ ,, odd, } \sigma \text{ ,, } \quad \text{,, } p \text{ even.}$$

In these equations the factors $a_p^\sigma b_p^\sigma$ are the coefficients of the Fourier series (see § 2), and the quantities r_n^σ, n_n^σ, m_n^σ, s_n^σ are numerical quantities, which (as well as their logarithms) are given in tables at the end of the paper as far as $n = 12$, $\sigma = 12$, $p = 12$. By means of these tables the numerical work is reduced to a minimum, and the coefficients of the series may be obtained as far as terms of the 12th degree.

8. The proposed method is specially adapted to deal with problems like that of terrestrial magnetism, in which the function to be obtained as a series of spherical harmonics is not given directly, but by means of its differential coefficients. The force directed to the geographical north may by Fourier's analysis be obtained as a sum, the terms of which have the form $\cos \sigma\phi \cos p\theta$, and $\sin \sigma\phi \cos p\theta$ when σ is even, and the form $\cos \sigma\phi \sin p\theta$, $\sin \sigma\phi \sin p\theta$ when σ is odd. Integrating with respect to θ, the magnetic potential is obtained in a form such that the transformation into the series of spherical harmonics may be proceeded with. A separate expression of the magnetic potential is derived from the force directed to the geographical east.

Contributions to a Theory of the Capillary Electrometer. II.—On an Improved Form of Instrument." By GEORGE J. BURCH, M.A. Oxon., F.R.S., Lecturer in Physics, University College, Reading. Received September 26,—Read November 20, 1902.

During the sixteen years that I have worked with the capillary lectrometer I have had occasion to make between 150 and 200 instruents, and have therefore given naturally a good deal of thought to he problem of its construction. I have used eleven different forms, hree of which are figured in my little book on the 'Capillary Elecometer in Theory and Practice,' reprinted from 'The Electrician,' 896. A fourth is in use in the Physiological Laboratory, Oxford, for he research on nerve in which Professor Gotch and I have been ngaged, and is in fact the improved form referred to on page 9 of my ook.

It may be of interest to indicate briefly the points that must be bserved in the design of the instrument.

It must be simple, easy to adjust and to clean, and with reasonable ence has shown that capillaries are less liable to become sticky with much acid than with very little. The only objection to this type is the difficulty of adjusting the capillary in so small a trough. The operation has to be effected under the microscope and is both delicate and tedious. When it has been completed the definition is perfect, and the instrument not unduly fragile. But in order to clean it the same process has to be gone through again.

I therefore determined to try an entirely different plan, on which the safety of the capillary should depend not on the rigidity of the supports by which the trough was fixed, but on their perfect flexibility and on the use of a trough so light that its entire weight might even be borne by the capillary.

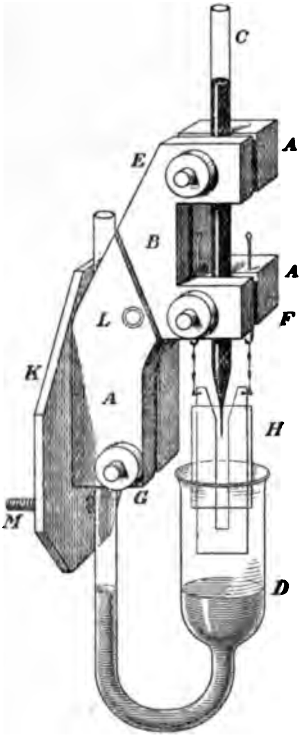


Fig. 1.—Half full size.

Fig. 1 is a perspective diagram of the instrument in its final form, and figs. 2, *a*, *b*, *c*, and *d*, show the details of the trough which is the essential part. The support A is cut from a solid block of ebonite 9 cm. long, 5 cm. wide, and 2 cm. thick. It is first cut to shape, holes drilled for the binding screws E and F, and the piece B then separated from it by two saw-cuts.

V-shaped grooves are cut to receive the capillary C, which is firmly clamped under B by E and F. The longer limb of the U-tube D passes through a hole drilled lengthways through the lower end of A, which is slit about half way up with a wide saw-cut, so that it may be pinched together by the screw G. Adjustments for setting the capillary at right angles to the optic axis and parallel to the slit of the photographic recording apparatus, are provided for by the stout brass plate K, bent at right angles, one end of which is fastened by a binding-screw at L to the back of A, and the other by a similar screw M to the adjustible stand of the projection microscope. The brass plate K is so shaped that there is a space of about 3 mm. between it and the left-hand side of the ebonite support A, in order to leave room for the adjustment of the latter about L as a centre.

The construction of the trough is shown in the full-size diagrams, figs. 2, *a*, *b*, *c*. A piece of mica, such as is used for lamp-shades, is cut to the shape *a*, with a pair of scissors. Two or three thicknesses may

e taken if one is not enough. Four holes are drilled with a needle in
he positions shown. A thin piece of the best clear mica is then laid
n a pad of blotting paper, the piece *a* placed on it, and four corre-
ponding holes pricked through with the needle, the piece being after-

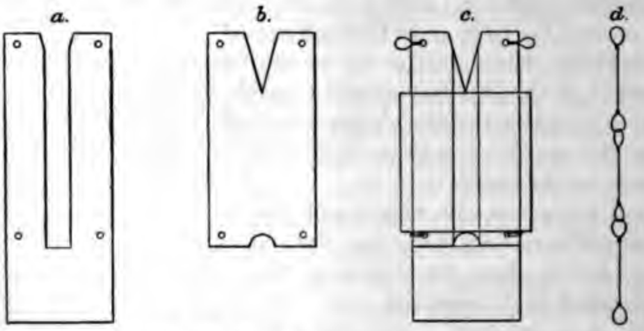


FIG. 2.—*a*, *b*, and *c*, full size; *d*, about twice full size.

ards cut to the shape *b*. Finally, *a* and *b* are fastened together by
ur little loops of No. 30 platinum wire, and the whole trimmed to

tilted back during each alteration. When these adjustments have been made, the inside of the trough is wetted by touching it with a glass rod dipped in dilute sulphuric acid of 25 per cent., and the apparatus is tilted forwards until the wet trough swings against the capillary, and sticks to it. A piece of thin cover-glass—or of mica if very high powers are to be used—slighly wider than the trough, is picked up with a pair of fine forceps, wetted on one side with the acid, and placed carefully against the trough, to which it adheres, holding it firmly against the capillary, the lower edge of the glass resting against the platinum loops with which the trough is fastened together (see fig. 2, *c*).

The U-tube is then turned back into position, cautiously raised until the lower edge of the glass just dips into the acid, and clamped by the screw G. Finally the trough is gently shaken or pushed to and fro in the plane of the mica, until the acid rises in it to the required height and all bubbles are expelled.

The trough is held together so firmly by surface-tension that it seems at first sight a difficult matter to take off the cover-glass without breaking the capillary. It may, however, be done with the greatest ease as follows: The screw G is loosened, and the U-tube D drawn down and turned aside. A small beaker filled with water is held in its place and raised until the trough H is completely immersed, when the slightest movement causes the glass to fall off. The apparatus is then tilted back so that the trough swings clear of the capillary, which may be washed or even wiped, and the trough dried, replaced, and a new cover-glass put on, in less than three minutes.

The definition, with these electrometers, is perfect. The capillary touches the cover-glass throughout its length, so that any dry objective can be used. The microscope should not, however, be left focussed on the capillary, lest the acid should chance to get between the objective and the cover-glass—an accident which I have known to happen in very damp weather.

My only fear in designing this instrument was lest contact with the mica should contaminate the acid and so spoil the tubes. There does not, however, seem to be any such effect. I have had some mica troughs in use for nearly three years, and have never once been troubled with a sticky capillary, and even with induction shocks they will stand more than the old type on account of the larger quantity of acid in the U-tube. They are easier to make than my old "normal type" on account of the straight capillary, and so far as I can judge they seem likely to supplant both it and my previous projection electrometer.

"On the Correlation of the Mental and Physical Characters in Man. Part II." By Alice Lee, D.Sc., Marie A. Lewenz, B.A., and Karl Pearson, F.R.S. Received November 3,—Read November 20, 1902.

(1.) In a first paper on this subject* we gave a brief account of our material—Miss Beeton's copies of the Cambridge anthropometric measurements with degrees added at the University Registry, and the school measurements carried out by assistance from the Government Grant Committee. This material will take years to exhaust, but the present notice gives further conclusions to be drawn from Dr. Lee's and Miss Lewenz's later reductions from this great mass of raw statistics.

(2.) In the first place we may refer to certain matters which arise directly from the first paper. In the discussion which followed the reading of that paper it was suggested that we ought not to correlate intelligence with absolute measurements on the head, but with their ratio to the size of the body. The answer made on that occasion was based on data not then published, namely, that there is no sensible correlation between intelligence and the absolute size of the body.

The v's and the physical correlations are due to Dr. W. R. Macdonell,* r_{iL}, r_{iB} were given in our first paper,† and r_{iS} was deduced from the following fourfold table:—

(A.) *Intelligence.*

		Honours.	Pass.	Totals.
Stature.	Over 69″	244	228·5	472·5
	Under 69″	280	258·5	538·5
	Totals	524	487	1011

If r_{iS} were really sensible, it would mean that honours men were slightly shorter than pass men. The only safe conclusion we can draw, however, is that stature is not correlated with place in degree examinations.

From the above results we find

$$v_{R_{SL}} = 4·1435, \qquad v_{R_{SB}} = 4·5530.$$

Hence we have

$$r_{iR_{SL}} = 0·0712, \qquad r_{iR_{SB}} = 0·0370.$$

That is to say, the correlations of intelligence with the ratios of length and breadth of head to stature are slightly smaller than the correlations of intelligence with the absolute head-measurements. The result predicted from the smallness of r_{iS} in the discussion on the paper here receives its exact numerical confirmation.

(3.) Since our school measurements were started, MM. Vaschide, and Pelletier have published in the 'Comptes Rendus'‡ a statement that although unable to find any relation between intelligence and length or breadth of head, they consider a relationship to hold between intelligence and the auricular height of head. Their process was of the following kind. They asked the school teacher to select ten intelligent and ten non-intelligent children, and then measured the heads of these two sets, and found their means. This was done for groups of three ages in boys and two ages in girls. The probable errors of the difference of the means of ten observations are not considered, and by exactly the same process that they reason that the auricular height is greater for the more intelligent children they might have deduced from their statistics that intelligent girls of 11 years have lower heads

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* 'Biometrika,' vol. 1, pp. 188-9.

† 'Roy. Soc. Proc.,' vol. 69, pp. 335–6.

‡ 'Comptes Rendus,' Paris, vol. 133, 1901, pp. 551—553.

nt girls of 9 years, and non-intelligent boys of 11 years
han the same class of 9 years! Frankly, we consider that
is a good illustration of how little can be safely argued
data and a defective statistical theory.

om our school data the auricular height of 2005 boys, and
owth table based on the same material, reducing them to
as standard, we find

(B.) *Auricular Height of Head and Intelligence.*

	Intelligent.	Slow.	Totals.
Above 127 mm.	481·5	584·0	1065·5
Below 127 mm.	415·0	524·5	939·5
Totals	896·5	1108·5	2005

e the correlation = 0·0161.
re is thus less correlation between auricular height and intelli-
than between either breadth or length and intelligence; indeed,
ess than the probable error, and no weight can be laid on it what-

The discovery of MM. Vaschide and Pelletier that the auricular
nt of school children is related to their intelligence seems to us
e incorrect for English boys, and unproven owing to defect of
and method even for French children.
sweeping critic, who clings to the *high*
school head-measure-

and half physical, they give with an equal weight the definite result that there is no *marked* correlation between intelligence and the size or shape of head in children.

(4.) While it seems desirable later to investigate specially the Cambridge data from the standpoint of the subject studied, as well as degree taken, we complete at present the list of other physical correlations with intelligence on the simple basis of honour and pass degree groups.

The following are the tables:—

Intelligence and Strength of Pull.

(C.) First Grouping.

		Honours.	Pass.	Totals.
Pull.	Above 84 lbs.........	251	256·5	507·5
	Below 84 lbs.........	273	229·5	502 5
	Totals	524	486	1010

(D.) Second Grouping.

		Honours, 1st class.	Honours, 2nd, 3rd classes, and Pass.	Totals.
Pull.	Above 84 lbs.	75	432·5	507·5
	Below 84 lbs.	78	424·5	502·5
	Totals	153	857	1010

Intelligence and strength correlation is from the first grouping $-0·0765$, and from the second $-0·0199$. Thus it would appear that from either grouping the honours men have slightly less strength of pull than the pass men, but as even this small amount is decreased when we group the first class men only together, such inferiority as there is seems to lie in the second and third class honours men. Taking the average, we may say that there is a negative correlation of $-0·0482$ between intelligence and strength of pull. The probable error of the result, about 0·035, shows that very little weight can be attached to it.

(E.) *Intelligence and Strength of Squeeze.*

		Honours.	Pass.	Totals.
Squeeze.	Above 85 lbs.	236·5	227·5	464
	Below 85 lbs.	282·5	255·5	538
	Totals	519	483	1002

The correlation between intelligence and strength in this case = − 0·0242.

This result, although it is less than its probable error, is again negative.

(F.) *Intelligence and Sight.*

This is judged in the Cambridge Anthropometric Laboratory by the distance at which the test type can be read.

Right eye.	Honour.	Pass.	Totals.

Now, it has sometimes been argued that in any investigation of this kind, it is desirable to take not absolute weight, but its ratio to stature or some power of stature. Let W = weight, S = stature, and n = any power; let $R_n = W/S^n$, and v be a coefficient of variation, and r one of correlation, i standing for intelligence.

Then

$$v^2_{R_n} = v^2_W + n^2 v^2_S - 2n v_W v_S r_{SW} \quad \text{.....................} \quad \text{(i)},$$

$$r_{iR_n} = \frac{v_W r_{iW} - n v_S r_{iS}}{v_{R_n}} \quad \text{..............................} \quad \text{(ii)}.$$

But

$v_S = 3{\cdot}6958$, $r_{SW} = 0{\cdot}4860$,

$v_W = 10{\cdot}8300$, $r_{iW} = 0{\cdot}0459$,

$r_{iS} = -0{\cdot}0058$,

from results already given for the Cambridge data. Hence, calculating v_{R_n} from (i) for n = 1, 2, and 3, we deduce

r_{iR_1} = correlation of intelligence with ratio weight to stature = 0·0540,

r_{iR_2} = ,, ,, ,, (stature)^2 = 0·0555,

r_{iR_3} = ,, ,, ,, (stature)^3 = 0·0503.

There is no substantial difference between any of these correlations and that for intelligence and absolute weight. As they were found indirectly by formulæ, it seemed desirable to test at least one of them directly. Accordingly Miss M. Beeton found the ratios of weight per inch of stature for 1012 Cambridge men. The resulting table was as follows:—

(H.) *Intelligence and Weight per inch of Stature.*

	Honours.	Pass.	Totals.
Over 2·224 lbs. per in.	258·5	222	480·5
Under 2·224 lbs. per inch ..	265·5	266	531·5
Totals....	524	488	1012

The distribution is sensibly the same as that of the table for absolute weights, and the correlation comes out 0·0604, *i.e.*, it differs only by 0·0064, or about one-fifth of the probable error from the value of the correlation obtained indirectly.

We may then, I think, conclude that whether we take absolute weights or the ratio of weight to stature, honours men are slightly heavier than poll-men. Summing up the whole of our examination thus far of the Cambridge measurements we may say that:

The honours men, and presumably therefore the more intelligent class, are slightly heavier and have slightly longer and broader heads; they are not quite as tall nor as strong, whether strength be measured by pull or squeeze, and are slightly shorter-sighted than the poll-men, or presumably the less intelligent class. In no single case, however, is the correlation between intelligence and the physical characters sufficiently large to enable us to group the honours men as a differentiated physical class, or to predict with even a moderate degree of probability intellectual capacity from the physical characters of the individual.

(5.) While the above and the previously published results exhaust the Cambridge data, as long as we preserve the division into honours and poll-men, much more remains to be done on this material when we consider subject groupings among the Cambridge graduates, or when we turn to the much wider range of both physical and mental characters recorded in our school measurements.

A preliminary inquiry may, however, be recorded here as bearing upon a rather vexed question at the present day, namely, the relation of athletics to health and intelligence. In our school measurements we had three categories: Health—divided into the classes: *Very Strong,* Strong, Normally Healthy, Rather Delicate, Very Delicate.* Ability or Intelligence—was divided into six classes: *Quick Intelligent, Intelli-*

The correlation dividing at the *Strong* is 0·0820.

The mean of the other divisions (i) dividing at the *Delicate*, and (ii) putting the *Slow Intelligent* with the *Intelligent*, gave 0·0835. We conclude, therefore, that there is a sensible, but not marked correlation between good health and intelligence.

Taking, however, health and athletics we have the table:—

(J.) *Health and Athletics.* 1743 *Boys.*

	Very strong.	Strong.	Normally healthy.	Rather delicate.	Very delicate.	Totals.
Athletic	91	447·5	497·5	120	3	1159
Non-athletic	9·5	98·5	293·5	166·5	16	584
Totals.....	100·5	546	791	286·5	19	1743

The correlation between healthy and athletic dividing between *Strong* and *Normally healthy* is = 0·4570, a very marked relationship.

Next, taking intelligence and athletics, we find:—

Intelligence and Athletics. 1708 *Boys.*

	Quick intelligent.	Intelligent.	Slow intelligent.	Slow.	Slow dull.	Very dull.	Totals.
Athletic ...	159·5	421·75	355·5	158·75	40·5	12	1148
Non-athletic	46	163·25	187·5	99·75	48·5	15	560
Totals.....	205·5	585	543	258·5	89	27	1708

Dividing between intelligent and slow intelligent we find the correlation between intelligence and athletic character is 0·2133.

This result may be exhibited also in the percentages of athletic and non-athletic boys who fall under each class of intelligence:—

Percentages of Athletic and Non-athletic Boys under each grade of Intelligence.

	Quick intelligent.	Intelligent.	Slow intelligent.	Slow.	Slow dull.	Very dull.
Athletic ...	14	37	31	13	4	1
Non-athletic	8	29	32	18	9	3

The relationship between keenness for combined with capacity in games and general intelligence is here manifest.

Certain other correlations with the athletic character may be just noticed without giving the tables. The athletic boy is popular (0·3250) and noisy (0·3452), and this although popularity is not found to be directly correlated with noise. He is slightly self-conscious (0·0761), and is more likely to be fair than dark (0·0391). His temper tends to be quick rather than sullen (0·2207), as the following table, based on 1664 cases, will show :—

Percentages of Athletic to Non-athletic Boys for each Temper.

	Quick tempered.	Good-natured.	Sullen.
Athletic	21	68	11
Non-athletic......	12	74	14

To sum up, then: While the intelligent are only *slightly* the more healthy, the athletic are *notably* the more healthy element in the community. Further the athletic are considerably more intelligent than

"Note upon Descending Intrinsic Spinal Tracts in the Mammalian Cord." By C. S. SHERRINGTON, M.A., M.D., F.R.S., and E. E. LASLETT, M.D. Vict. Received November 5,—Read November 27, 1902.

In the course of experiments upon the paths of nervous conduction in the spinal cord of the mammal, one of us observed* very numerous and wide departures from the fourth so-called "law" of Pflüger. That "law" states that "Reflex-irradiation in dem Rückenmarke nach Oben, resp. Vorn, gerichtet ist; also gegen die Medulla oblongata."† The observation of the above-mentioned exceptions rendered desirable a search for more detailed evidence of intrinsic spinal paths running in the aboral direction. We therefore set about inquiring into the existence of spinal paths connecting the activity of segments situate nearer the head with segments lying further from the head. Such evidence is obtainable with some experimental difficulty, but it has been eventually forthcoming, and amounts to demonstration of the microscopic course of the channels involved. It is some main features of these latter that we desire to record in the present communication.

METHOD.

The method employed has been that of the Wallerian nerve-fibre degeneration, but with a novel feature in the mode of application of the method. For the purpose in view the ordinary establishment of a cross-lesion in the spinal cord is futile. The secondary degeneration then produced befalls, in the spinal region under investigation, all nerve-fibres having their perikarya headward of the cross-lesion, whether those perikarya lie in the cerebral hemisphere, basal ganglia, mid-brain, cerebellum, bulb, or cord itself. It is obviously then impossible to identify which particular ones, if any, of the degenerate nerve-fibres are coming from the cord-segments whose nerve-tracts are the special object of inquiry. To obviate this difficulty, we have adopted a method which may be termed a method of "*successive degeneration*." The method consists in producing two or more successive degenerations with allowance of a considerable interval of time between them. In the piece of cord to be examined, a first degeneration is allowed time enough to remove all the tracts descending from sources other than those the immediate object of inquiry. This is a procedure which requires in our material, at shortest, 9 or 10 months to complete. When the time is complete, the cord is left, as it were, like a cleaned slate, on which once more a new degeneration can be written without fear of confusion with a previous one. The cord is then ready for receiving the lesion which shall cause degeneration of the particular tracts whose existence is suspected. After a period suitable for the full development of the new degeneration, the cord is treated histologically by the Marchi method, and the microscopical examination proceeded to. This method resembles in principle a method employed with noteworthy results by Münzer.* This author performed on the new-born rabbit a first lesion (*e.g.*, removal of one cerebral hemisphere, injury of mid-brain or cord), and later, in the animal when grown, proceeded to establish a new lesion which was thus uncomplicated by the part already separated: thus "Gudden's *agenesic* atrophy" was made to precede the degeneration desired for study.

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* "Croonian Lecture," 'Phil. Trans.,' B, 1897.

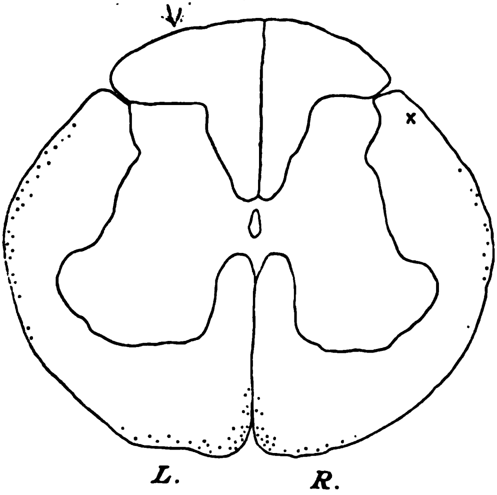
† 'Die sensorischen Functionen des Rückenmarks der Wirbelthiere, nebst einer neuen Lehre über die Leitungsgesetze der Reflexionen' (p. 73), Berlin, 1853.

One of the experiments made by Münzer and Wiener (1895) deals with the problem undertaken by ourselves. After semi-section of the spinal cord of the new-born rabbit at the last dorsal segment, they performed total transection two segments further back when the animal was grown. Behind the second section they found "as many fibres degenerate on the semi-sected side as on the intact side." If not "decusthe cord is ripening for the second operation. We have, however, obtained thirteen successful complete experiments: in these the shortest interval has been 260 days, the longest 568 days. Among these is included two in which, for special reasons, the partial cross-lesion of the cord was made precedent to the total transection.

RESULTS.

The spinal segments examined as sources of aborally-running fibre-systems have been posterior cervical, anterior thoracic, mid thoracic, posterior thoracic, and anterior lumbar. From all these regions our experiments demonstrate that copious aborally-running fibre-systems spring. Thus, the accompanying fig. 1 shows, for instance, tracts of fibres in the 5th lumbar segment which have their origin in cells of the 2nd thoracic segment.

FIG. 1.



Cross-section of the cord of the dog at a level in the anterior part of the 5th lumbar segment; Marchi preparation. The section reveals the topography at that level of the aborally-running fibre-system of the 1st and 2nd thoracic segments. The 8th cervical segment of the cord had been completely exsected and ablated. A partial translesion (rather more than a semi-section) was made the left half of the 3rd thoracic 568 days subsequent to the removal of the 8th segment. The exact extent of this second lesion was determined subsequently by microscopic examination in serial preparations, and its limits will be described and figured in a fuller communication. The dots indicate, in a way mentioned in the text (p. 120), the density and extent of the degenerate tracts of fibres. L = left side; R = right side.

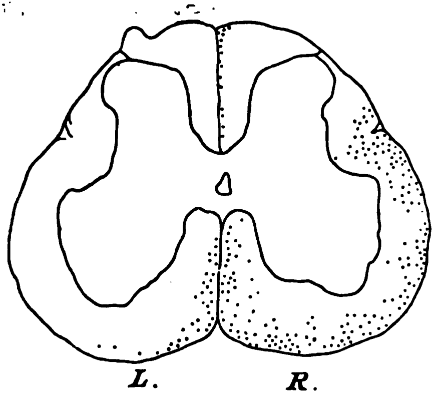
Speaking generally, of the fibres composing the aborally-running systems springing from the grey matter of the spinal segments examined, we find there may be distinguished two sets. For physiological description it is in some ways convenient to regard the length of the spinal cord as divisible into regions; thus, a brachial for the fore limb, a thoracic for the trunk, a crural for the hind limb, a pelvic for pelvic organs, a caudal for the tail, and so on. A reflex initiated *viâ* an afferent path of one such spinal region may evoke its peripheral effect by efferent paths of a spinal region other than that to which the original entrant path belongs. Such a reflex has in a former paper by one of us* been termed a "long" spinal reflex, in contradistinction to reflexes whose centripetal and centrifugal paths both belong to one and the same spinal region. The latter reflex it was proposed to term "short."† Analogously, in the aborally-running fibre-systems of the spinal segments examined, by our experiments fibres of two categories are found, one a set passing beyond the limits of the spinal region in which they arise, the other not passing beyond those limits. The former we would term "long spinal," the latter "short spinal" fibres. In each of these main categories there can be distinguished fibres of various intermediate length.

Again, the fibres of each of the above two categories may be classified into two sets or tracts, according to their topography relatively to the cross-section of the cord. Fibres of both of the above categories short fibres; but analysis of our material makes us hesitate to positively affirm that it is true for all of them. A small proportion of the short fibres *may* decussate, at least in the sense that short fibres arising in perikarya belonging to one lateral half of the cord may find their way into the white ventrolateral columns of the crossed half. We do not affirm, however, that any of even our "short" fibres do decussate, we simply affirm our present inability to deny that a small proportion of them may do so.

Some of the "long" fibres are very long, both in the lateral and in the ventral columns of the cord. Thus, some of those arising from perikarya in the 6th and 7th cervical segments we have traced into the sacral region, *i.e.*, through nearly thirty spinal segments, both in the lateral and in the ventral columns. The rule pointed out by one of us in a previous paper,* that the long fibres in the spinal cord tend to lie nearest the surface of the cord, is well exemplified in these intrinsic spinal systems.

Besides fibres in the ventrolateral columns the aborally-running fibre-systems of spinal origin include fibres in the dorsal columns.

FIG. 2.



Cross-section of the spinal cord of the dog at the level of the 1st sacral segment; Marchi preparation. The section reveals the topography at that level of the aborally-running fibre-system of the nerve-cells of the 1st and 2nd lumbar segments. A short length of the 13th thoracic segment of the cord had been completely exsected and ablated. A partial translesion (rather more than a semi-section) was then made through the right side of the 2nd lumbar segment in its anterior levels 290 days subsequent to the total exsection. The extent of this second lesion was accurately determined later by microscopic examination in serial preparations; its exact limits will be described in a fuller communication. The dots indicate, in the way mentioned in the text, the density and extent of the tracts of degenerate fibres. L = left side; R = right side.

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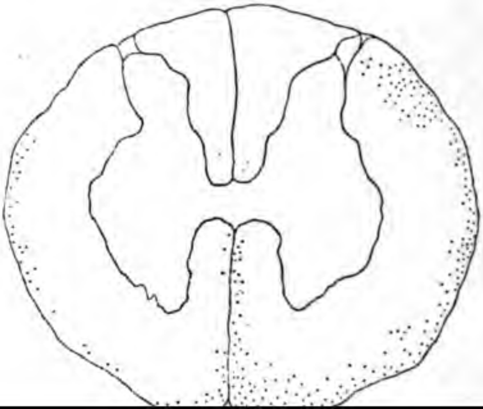
* 'Journ. of Physiology,' vol. 14, p. 298. Cambridge and London, 1893.

These are less numerous. We defer detailed description of them, together with further detailed description of the ventral and lateral tracts above-mentioned, until a fuller communication dealing with the whole subject. The general features of the topography can be gathered better from the two accompanying figures than from any even lengthy

Fig. 3.



of it. Extreme care has been taken, however, to make the number of the dots bear fairly accurately a general proportion to the density of the degeneration, and the same proportion in one drawing as in another.

"The Inter-relationship of Variola and Vaccinia." By S. MONCKTON COPEMAN, M.A., M.D. Cantab., F.R.C.P. Communicated by LORD LISTER, F.R.S. Received November 13,—Read November 27, 1902.

The term "variolæ vaccinæ" employed by Jenner, as a synonym for cow-pox, has been generally accepted as affording evidence that in so naming this disease, "small-pox of the cow," he was desirous of placing on record his belief that cow-pox, or vaccinia, was intimately related to human small-pox, if indeed it were not directly derived from it.

This theory, however, appears to have found but scanty favour in Jenner's day, and even at the present time the value of the practice of vaccination is, by some, impugned on the plea that inoculation of one disease—cow-pox—could not be expected to exert any really protective influence against the ravages of small-pox—a disease considered by them of totally different origin.

In the hope of obtaining definite information on the subject, many observers, during the long period which has elapsed since the introduction of vaccination, have set themselves the task of attempting to solve, by experimental methods, the problem of the true relationship of vaccinia to variola.

These attempts have been, for the most part, directed to the possibility of giving rise to cow-pox by the introduction, in one or another manner, of the virus of small-pox into the system of the bovine animal. In the great majority of such attempts, which have been much more numerous than is generally supposed, the results have been entirely negative, although so numerous have been the experimenters, who from time to time have attacked the problem, that the total number of instances in which an apparently successful result has been obtained, is now considerable.

So far as I am aware, the first recorded experiments are those of Gassner of Gunsberg, who, in 1801, succeeded, after no less than ten fruitless attempts, in directly variolating a cow with small-pox virus. The lymph thus obtained was employed for the vaccination of four children, from whom other seventeen were subsequently vaccinated. None of these exhibited any signs of small-pox.

It is impossible, here, to do more than mention the names of other investigators who have engaged in research of this nature at various times from the commencement of the last century up to the present time. I therefore merely append in the foot-note* a list of such names, placed, as far as possible, in chronological order. It is a noteworthy fact that every observer mentioned, with the exception of Chauveau and his colleagues of the Lyons Commission, and Martin, claim to have obtained, on one or more occasions, positive results as regards the production of typical vaccinia, generally after one or more removes from the animal originally variolated.

But it must, I think, be admitted that many of the earlier experiments, more particularly, are practically worthless owing to the conditions under which they were carried out. Some of the main objections are based on the frequently concomitant use of vaccine and of variolous lymph on the same animal, and the want of care as to the cleanliness and freedom from vaccine contamination of lancets and "points" used in the experiments.

As regards my own work, carried out on similar lines to those adopted by previous observers, and of which a full account was published in the 'Journal of Pathology and Bacteriology,' in 1894, it may here be mentioned that I obtained an undoubtedly successful present paper, my attempts at direct transference of human small-pox material to the calf met with no success.

All my earlier experiments were conducted at the Brown Institution, in order to avoid any possibility of contamination with vaccinia. As a further precaution new scalpels were used, which were invariably first carefully sterilised in the flame of a spirit lamp, and, after use, the table was, on each occasion, thoroughly washed with carbolic acid (1 in 20), while during the intervals of use it was kept exposed to the air under an open shed. Similar precautionary measures have been observed throughout the course of my later work.

The difficulty experienced by myself and the numerous other investigators, to whom reference has already been made, in attempts to transmit human small-pox directly to bovines, whether cows or calves, is not infrequently cited as a reason for regarding with distrust the theory expounded by Jenner, that cow-pox, whether carried through the horse as intermediary host or not, was originally derived from small-pox in the human being.

But a great deal, at any rate, of the small-pox which was prevalent at the time that Jenner lived and wrote was of that comparatively mild variety which, under the name of inoculated small-pox, was intentionally produced in healthy subjects, with the object of thereby conferring protection against subsequent attack by the disease in virulent form.

So mild indeed at times were the results of inoculations in the hands of such operators as Adams and the brothers Sutton that, as we learn from contemporary records, in many instances but little obvious effect was observed, with the exception of the local vesicle arising at the site of insertion of the small-pox virus, and the patients suffered but little inconvenience. Thus, more particularly in certain of Adams' cases, as may be gathered from his own account of the circumstances, the visible effect produced so closely resembled the results then beginning to be known as following on the Jennerian process of vaccination, that numbers of his patients were with difficulty persuaded that he had not, contrary to their desire, intentionally vaccinated rather than variolated them. The gradual evolution of a strain of lymph of such tenuity, according to Adams himself, was obtained by attention to the mode of life and general treatment of persons undergoing the process, together with careful selection of the sources (preferably the primary vesicle) from which the virus was obtained.

The majority of persons thus inoculated are not likely to have been incapacitated, as the result of the operation, to a much greater extent than are those who undergo efficient vaccination at the present day, and doubtless, therefore, they would be, for the most part, capable of following their ordinary avocations during the progress of the induced disorder. On the other hand, this would hardly have been possible in the case of persons contracting small-pox in the ordinary way, among whom the disease was apt to exhibit such virulence as to account for the death of perhaps 50 per cent. of those attacked.

Not only were the effects following on inoculation comparatively mild, but the disease in this form was intentionally brought into many country districts which otherwise might not have become invaded by small-pox. In the light of these facts, it has for some time past been borne in upon my mind more and more convincingly that it was probably from the *inoculated* form of small-pox, rather than from the ordinary variety of the malady, that much, at any rate, of the cow-pox, in the pre-vaccination era, was derived. It is not difficult to understand how that the cracks so often found on the udders of cows might become infected by a milker with fingers contaminated by contact with the inoculation sore upon his arm.

I determined, therefore, if possible, to put the matter to the test, and, learning that in Nubia, in Burmah, and in certain parts of India the inoculation of small-pox is still practised, I made numerous endeavours to obtain the necessary material, but unfortunately without success.

In default, therefore, of inoculated small-pox in the human subject, with a sterilised lancet, and their fluid contents received into fine capillary tubes, which were subsequently sealed in the flame of a spirit lamp to admit of transport. This operation, however, is a most laborious one, and was subsequently abandoned, at my suggestion, in favour of collection, in the post-mortem room, of vesicle pulp at a suitable stage of the eruption, by means of a small Volkmann's spoon, after the fashion now invariably used in the Government lymph laboratories in obtaining, from the calf, material for the production of glycerinated lymph.

After removal from the body the small-pox pulp is first carefully weighed, and then ground up in a small glass mortar, with the gradual addition of usually four times its weight of a sterilised 50 per cent. solution of pure glycerine in normal saline solution. After thorough emulsification, what is not required for immediate use is stored in tubes, resembling small test-tubes, which are then corked, sealed with liquefied paraffin to which carbolic acid has been added, and set aside in a chamber kept at a temperature a few degrees above freezing point. Both storage-tubes and corks are sterilised before use.

Bacteriological examination by the method of plate-culture often shows a comparatively small number of extraneous micro-organisms in a specimen of small-pox emulsion prepared in the manner described, but whenever possible it has been stored at a temperature of about 15° C. for some weeks prior to using it for inoculation.

Species and Age of Monkeys Inoculated.

For my original experiments on the transference of human small-pox to the monkey, a brief account of which was presented to the Royal Society in 1893, I employed the rhœsus monkey, for the reason that Professor Sherrington and myself had, at the time, a stock of these animals, which had been obtained for other experimental work. Having at that time obtained successful results in every one of my inoculations, I employed the same species of monkey in the greater number of the experiments comprised in the present research. As, however, during the progress of the work I learnt that Dr. Eilerts de Haan, who, in Batavia, had been working on similar lines to myself, had made use most successfully of the macaque monkey, I also obtained a few specimens of this species, in order to compare the results of variolation in these animals with those that I had previously observed in the rhœsus monkey. But after two or three inoculations of the macaque with small-pox material, I came to the conclusion that the results following on the operation were not ordinarily as typical as in those experiments in which rhœsus monkeys had been employed. At the same time the macaque is in this country more expensive and more difficult to obtain than the rhœsus, so that I reverted to the use of the latter species in subsequent work.

It would appear also that, as in the human subject, young animals are more susceptible to small-pox than are adults, since it was in those instances in which monkeys probably not more than a year old were variolated that the most successful results were obtained. In one instance, however, in which the monkey was believed to be not more than a few months old, the extremely fine downy hair, after shaving, grew again so rapidly as to render somewhat difficult the photographing of the effect produced by the operation.

Mode of Operation and Collection.

In the earlier experiments inoculation of the monkey with human small-pox emulsion was carried out by rubbing it well into scarified patches or linear incisions of the skin of the upper arm or of the inside of the thigh, after previous shaving and cleansing of the skin. Subsequently, however, in accordance with the suggestion of Dr. de Haan, a shaved area on the back of the animal was utilised for inoculation. In this situation the results of the operation were found to be equally good, and there is less liability of damage to the vesicles from the monkey scratching itself.

The eruption having arrived at maturity, after the lapse of a period ment. They were fed and otherwise attended to by a man specially detailed for the purpose. All instruments employed for the vaccination of monkeys, calves, or children were previously sterilised by boiling or passing through the flame of a spirit lamp. Attention may perhaps be called to the fact that the skin of the scrotum in the calf affords a specially favourable site for inoculation experiments, especially if, when the incisions are made, the skin is made tense by pressing down the testicles. The first transference from the monkey to the calf does not usually afford a perfect result. Indeed a second, third, or even later passage from calf to calf was usually required before the most typical vesiculation was obtained.

In certain cases children were vaccinated with lymph obtained from the experimental calves, and in all instances the resulting vaccination ran a perfectly normal course. With lymph of similar origin I also successfully vaccinated myself. But none of the strains of vaccine lymph, derived originally from human small-pox in the manner described, have been brought into general use.

PROTOCOLS OF EXPERIMENTS.

First Series.

February 21, 1898.—Glycerinated samples of small-pox lymph received this day from Medical Officer of Health of Middlesbrough. Patients living and aged respectively 20, 27, and 34 years; all had been vaccinated in infancy.

April 1.—At Brown Institution, inoculated small rhœsus monkey with small-pox emulsion of 21.ii.98 in five linear incisions on left arm, and in fourteen on abdomen, after previous shaving and cleansing of the skin, by means of soap and water, followed by warm boric acid lotion. Monkey isolated in separate room and attendant vaccinated as a precautionary measure.

April 5.—All insertions on both arm and abdomen evidently "taking."

April 8.—Distinct vesiculation at site of all incisions on arm and most of those on abdomen. Monkey ætherised and substance of vesicles removed with sharp spoon into small previously weighed and sterilised test-tube. Scrapings weighed (0·6 gramme) and ground up with six times the weight of 50 per cent. watery solution of glycerine. Resulting emulsion taken up into twelve capillary glass tubes.

April 13.—Monkey looks well. All incisions healed up. No sign of generalised eruption.

Calf Experiments.

April 9.—At the Animal Vaccine Establishment, Mr. Stott inoculated Calf No. 1 (No. 4363) on two scarified patches, in twelve incisions on scrotum, and forty-four in perineum and on abdomen, with contents of two capillary tubes of glycerinated pulp prepared from vesicles of monkey. Incisions made with scalpel previously sterilised.

April 12 (72 hours).—Practically nothing to be seen.

April 14 (120 hours).—All insertions on scrotum appear to have "taken," and, in addition, four (not quite so well) on abdomen. Insertions on perineum seem to have failed. Large bullous-looking vesicle on upper scarified patch.

From this and from the vesicular lines on scrotum and abdomen collected pulp by scraping, after clamping with compression forceps.

The same day (April 14), Calf No. 2 (No. 4369 in A.V.E. records) was inoculated on perineum, scrotum, and abdomen with material obtained from Calf No. 1.

April 19.—All inoculated lines, with exception of two on abdomen, "taken" well, eruption being markedly vesicular. Vesicles clamped and scraped; pulp being immediately employed for inoculation of Calf No. 3 (No. 4673 in A.V.E. records) in a number of long incisions on the perineum, scrotum, and abdomen.

April 24.—All lines of incision "taken" well; eruption perfectly typical of vaccinia.

From this calf, six children vaccinated at A.V.E. same day.

May 1.—Children returned for inspection, in ordinary course. All vaccinations completely successful. Photographed arms of two of these children, which presented most perfect eruption.

About a month later I hunted up the parents of all six children, when I learnt from the mothers' statements that in every case the vaccination had pursued a perfectly normal course.

SECOND SERIES.

March 3, 1900.—At the West Ham Borough Hospital, Dagenham, I removed small-pox vesicles from body of a man, æt. 56, who had died 24 hours previously from semi-confluent form of disease. Material removed in test-tube and placed in ice-chest.

March 5.—After removal of some shreds of epithelium for histological purfor inoculation in twelve insertions of *Monkey No.* 4 (young male rhœsus) same day.

April 2.—All places have "taken" well, although material used for inoculation had been so diluted. Photographed. Vesicles scraped and pulp glycerinated.

Calf Experiments.

March 12.—Inoculated Calf No. 1 in half dozen long linear incisions by method usually employed in current vaccinations at Government Establishment, with glycerinated pulp from Monkey No. 1.

March 16.—Lines of incision slightly raised and red.

March 17.—Dr. Fremlin found few small vesicles had developed. These he clamped, inserting material obtained thereby on Calf No. 2.

March 21.—There were evident signs of "taking" at all points of insertion, the lines of incision being elevated and with a tendency to vesiculation. But appearances not considered sufficiently typical to permit of material removed being utilised for vaccination of children.

At this point, owing to unforeseen circumstances, this particular series of experiments was discontinued.

THIRD SERIES.

February 25, 1901.—At the Jenner Institute inoculated medium-sized macaque monkey with small-pox emulsion, made by working up scrapings from P.M. cases of the disease (received from Dr. Thomson, of the Belvedere Hospital, Glasgow), in a small amount of pure glycerine. Technique as in previous experiments. Incisions made on the monkey's back with blunt scalpel, which had lost its temper by constant passing through the flame, so that all incisions did not apparently penetrate to the true skin.

March 4.—"Taken" well, though not throughout all insertions as in first monkey of the last series, but as failure had only occurred where there was no mark of incision, it was probably for reason mentioned above, as the eruption which had appeared was good. Not markedly vesicular; lines of incision which were covered with slight crust being surrounded by a pinkish papular eruption. Photograph taken.

Scraped with aid of compression forceps, and rubbed up material in small amount of NaCl 0·7 per cent. solution.

Monkey No. 2 inoculated this day, with emulsion of scrapings from Monkey No. 1. Technique as before.

March 16.—All insertions "taken"; slightly more vesicular than in No. 1. Photographed. Compression forceps applied and scrapings removed and rubbed up in small mortar with NaCl solution.

Monkey No. 3 inoculated immediately, on the back, with emulsion of material obtained from No. 2. Animal very young; hair downy and not easily shaved.

March 18.—All insertions have "taken" and have wide whitish vesicular margin, but appearance rather spoiled as hair on back has grown so rapidly. Lymph oozed up when compression forceps applied. Photographed. Scrapings rubbed up with NaCl solution, of which small quantity was used immediately for inoculation of Monkey No. 4. Remainder glycerinated, tubed and stored in ice-chest for future trial on calf.

March 23.—Monkey No. 4, an old animal, had not apparently taken as well as No. 3, so series was discontinued.

April 20.—Monkey No. 3 vaccinated in six incisions on outside of thigh with current vaccine lymph of known potency.

April 27.—No result, although monkeys not previously protected take vaccinia as successfully as in the human subject.*

Calf Experiments.

March 22.—Calf No. 1 (1332), at the Government Animal Vaccine Establishment, inoculated with small quantity of glycerinated emulsion of vesicular pulp from Monkey No. 3.

March 27.—Tiny papules and vesicles which had made their appearance along lines of incisions removed with Volkmann's spoon and glycerinated.

March 29.—Material collected on March 27 inserted into three long incisions on Calf No. 2 (1342).

April 3.—Fifth day. Lines of all three incisions occupied by good vesicles, Photograph taken by Dr. Green. Vesicle pulp (0·37 gramme) removed and glycerinated.

April 24.—Calf No. 3 (1390), inoculated (with portion of material collected on April 3) in thirty-six incisions on abdomen and scrotum.

April 29.—All insertions had "taken" well, vesicles surrounded with slight pink areola. Vesicles scraped and pulp glycerinated. Emulsion stored in ice-chest.

October 3.—Four c.c. of this emulsion used for vaccination of calf at the Jenner Institute, by numerous linear incisions extending nearly whole length of abdomen, after manner usually employed at Government stations.

October 8.—Appearance indistinguishable from normal vaccination. 63 grammes of vesicle pulp collected and glycerinated.

The glycerinated emulsion prepared from material removed from the calf on October 8 was subsequently employed for the vaccination of other calves, a strain of lymph being thus obtained which continued to give excellent results both on children and calves. But the strain was never brought into general use, and all the glycerinated emulsion remaining was eventually destroyed.

FOURTH SERIES.

April 29, 1901.—At the Jenner Institute young rhœsus monkey shaved on back, as in previous experiments, and inoculated in a dozen linear incisions with glycerinated emulsion of S.P. vesicle pulp received from Dr. Thomson, of the Belvedere Hospital, Glasgow, on March 26, 1901.

April 2 (120 *hours*).—Had "taken" so well that I decided to collect; lines of incision distinctly vesicular. After taking photo, washed inoculated area, and removed pulp with aid of clamp forceps. Thin lines of altered epithelium came off as in a good calf vaccination. No "crusting."

Monkey No. 2 inoculated same day with material obtained as above and subsequently triturated in small glass mortar with small quantity of equal parts of glycerine and normal saline solution.

May 9 (120 *hours*).—Every insertion "taken" successfully. More markedly vesicular in places than Monkey No. 1. Photograph taken, followed by usual process of collection and glycerination af vesicle pulp.

Monkey No. 3 inoculated on back in eight diagonal incisions. Emulsion remaining over taken up into capillary tubes, of which two given to Dr. Fremlin for trial on calf at A.V.E. on May 15.

May 13 (120 *hours*).—Eruption of perfectly vesicular character along course of all incisions made, the centre in each instance being occupied by thin linear crust.

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* *Copeman*, 'Journal of Pathology and Bacteriology,' May, 1894.

Drs. Blaxall and Fremlin, on seeing the animal, both described eruption as being equal to that which in case of calf vaccination would be entered in official records as v.g. (very good). Monkey photographed and vesicle pulp collected and glycerinated. Portion of emulsion used same day for inoculation of Monkey No. 4. Another portion handed over to Dr. Fremlin for trial on calf at A.V.E.

May 18.—Monkey No. 4 "taken" well. Vesicle pulp collected, emulsified, tubed and stored in ice-chest.

Series not continued beyond this stage, as laboratory man had failed in attempts to obtain further supply of young rhœsus monkeys.

Calf Experiments.

May 8.—Emulsion of vesicle pulp from *Monkey No.* 1, used at A.V.E. for inoculation of Calf No. 1418.

May 13.—Dr. Blaxall noted " No vesiculation, slight thickening in one line."

May 15.—Material from Monkeys No. 2 and No. 3, inoculated on calves at A.V.E. by Dr. Fremlin.

May 20.—Both calves had "taken" to a certain extent, the result being most marked in calf inoculated from Monkey No. 3. Material collected, glycerinated, and stored. Portion subsequently handed to Medical Director of Jenner Institute for further trial.

May 26.—Inoculated calf at A.V.E. with emulsion of second removal from Monkey No. 3.

May 31.—Perfect vesicular eruption along course of all incisions. General effect indistinguishable from that obtained with the current vaccine lymph of the Government Establishment.

In view of successful results following on vaccination of children with former lymph stocks raised in similar fashion, it appeared unnecessary to employ this particular lymph for infantile vaccinations. But on vaccinating my own arm with it direct from the calf, I succeeded in raising by the eighth day a fairly typical vesicle, an effect in excess of that obtained by me on my own person at previous attempts at vaccination.

No further transference of this lymph was attempted.

My first series of experiments had not long been concluded, when I came across a reference to an account of similar work which had been carried out by Dr. Eilerts de Haan. The reference occurred in a paper by Dr. Bruno Galli-Valerio,[*] and on hunting up Dr. de Haan's original paper, which is entitled, "Vaccine et Rétrovaccine à Batavia,"[†] I found that it contained an account of a lengthy series of experiments on the variolation of monkeys and on the transference of the resulting affection to calves.

Dr. de Haan's work proved of special interest to me for the reason that, quite independently, we had been able to corroborate one anothers' work, except as regards the transference of the strain of variola vaccine to the human subject—a final test which Dr. Eilerts de Haan did not, as he says, feel justified in attempting, in view of the unfortunate experience of Chauveau, in connection with his abortive tempts at variolation of the cow. Dr. de Haan's own words may be oted:—"Je reconnais qu'il manque à ma démonstration d'avoir pporté la variole mitigée du singe sur l'homme ; c'est une expérience e je ne me suis pas cru en droit de faire. L'expérience de Chauveau seigne à être prudent, et je ne me croirais autorisé à faire cette ntative que si le vaccin ordinaire dont je me sers me manquait au oment d'une épidémie. Mais j'espère qu'on répétera mes expériences ce sujet."

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[*] 'Centralblatt für Bakteriologie,' March 28. 1899, p. 380 *et seq.*

[†] 'Annales de l'Institut Pasteur,' 1896, p. 169.

For the purpose of his experiments Dr. de Haan made use of the acaque monkey (*Macacus cynomolgus*), which is common in the utch East Indies, and therefore was readily obtainable. His inocula- ons were, in each instance, made on a portion of the animal's back, hich was first shaved and then cleansed with soap and water, llowed by a solution of boric and salicylic acids. In his first ries of variolations of the monkey, the small-pox lymph employed as obtained from a Javanese child, no statement however being ade by him as to the age of the child, whether or not it had ever een vaccinated, or at what stage of the disease the lymph was taken. ransferred to the monkey, this lymph gave rise, in a week's time, to ell-marked vesicles at the site of inoculation, while, in addition, few papules were observed on the lips and the extremities. Seven

In each of the separate series of experiments the human small-pox lymph or pulp was first inoculated directly on calves, and in every instance, so far as could be observed, with altogether negative results. But with monkeys success was as invariably obtained, and when, after one or more passages through this animal, the contents of the local inoculation vesicles were employed for insertion on the calf, an effect was now produced which, after two or three removes in that animal, was indistinguishable from typical vaccinia.

Moreover, from the contents of vesicles raised in this manner on the calf, a number of children have, in turn, been vaccinated, some of whom were afterwards kept under observation for as long a period as a couple of months.

Every such vaccination "took" normally, and in no case was any bad result subsequently observed by myself, or reported by the parents of the children; no "generalisation" of the eruption occurring in any instance.

In conclusion, I desire to call attention to the somewhat remarkable fact that a mild and strictly localised form of small-pox, such as is induced in the monkey by the inoculation of material from cases of the generalised disease in man, should, when transferred to the calf, "take" readily with the production of a vesicular eruption of non-infectious character in that animal, whereas it is well known that successful transference of small-pox direct from man to calf can only be accomplished with the utmost difficulty.

The experimental results obtained in the course of the research, an account of which has been set out in this paper, all tend, then, to confirm the view that the vaccinia of Jenner's time was derived, in all probability, from a comparatively mild form of human small-pox.

In addition, I think it will be admitted that the work has afforded conclusive evidence of the essential identity of the virus of small-pox and cow-pox or vaccinia.

"On the Similarity of the Short-period Pressure Variation over Large Areas."

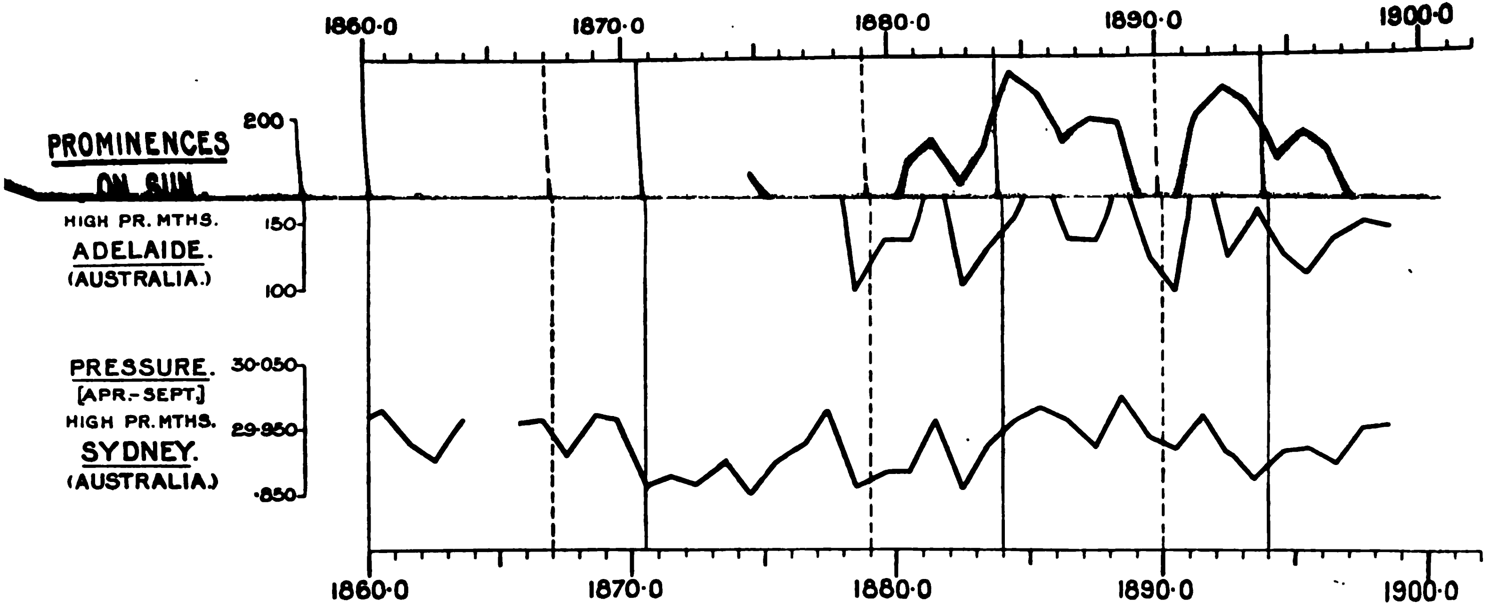
By Sir NORMAN LOCKYER, K.C.B., F.R.S., and WILLIAM J. S. LOCKYER, M.A., Ph.D., F.R.A.S. Received October 18,—Read December 4, 1902.

[PLATES 1 and 2.]

In a paper presented in June last to the Society,* we pointed out the existence of a short-period oscillation of barometric pressure over the Indian area corresponding generally with a variation in the percentage number of prominences recorded on the sun's limb. This oscillation was further shown not to be limited to the Indian area, but to be marked at a far distant station, as Cordoba, in South America.

The present paper, which is a continuation of this investigation, was undertaken to extend the research over a larger area.

The monthly means of the pressure variations for each station have been divided as previously into two periods, namely, those months in which the pressures are above and those in which they are below the normal, the normal being the mean pressure for the whole period under investigation in each locality.



Note.—The vertical continuous and broken lines represent the epochs of maxima and minima of sunspot activity.

region on both sides of the equator, it was important to study the extent of the region in the New World in which pressure variations similar to those of Cordoba had been recorded.

As Cordoba represents an area south of the equator, a portion of the United States of America was taken as typifying an area with north latitude and in about the same longitude, and a commencement was made along the lowest available parallel of latitude. This was rendered possible by the kindness of Professor F. Bigelow, of the Weather Bureau, who very generously forwarded proof-sheets of a new reduction of the pressures of many stations. We wish to take this opportunity of expressing to him our best thanks.

Treating these pressures in the same way as those formerly investigated in the Indian region, several stations which had the best record were chosen. A graphical representation of the variations of four of these stations (Mobile, Alabama; Jacksonville and Pensacola, Florida; San Diego, California) is given in Plate 2, and for the sake of comparison the pressure of Cordoba, with the inverted curves representing the Bombay pressure and solar prominence variation. This series of curves refers in all cases to the variations of the means of the high pressure (winter) months (October to March in most cases). At Cordoba, which has a southern latitude, the high pressure months extend from April to September.

The result of the comparison shows that in this region of the world we have a large area, the pressure variations of which are strikingly similar to, but are the inverse of, those recorded in nearly the antipodal part of the globe.

The facts observed are so suggestive that we are continuing the inquiry by collecting and discussing observations made in other areas.

Although the general agreement between the two main sets of curves is most striking, there are minor differences which probably, as stated in the previous paper, will eventually help us to determine those cases in which the prominence effects on pressure are masked by some special local conditions. It may be added that the available observations of prominences refer more directly to their quantity than to their intensity.

We wish to express our thanks to Mr. W. N. Shaw, F.R.S., who kindly placed the records of the Meteorological Office at our disposal; and to Mr. Hodgson, who has extracted the requisite data from the available records of pressure, and constructed some of the curves.

n the Vibrations and Stability of a Gravitating Planet." By J. H. JEANS, B.A., Isaac Newton Student and Fellow of Trinity College, Cambridge. Communicated by Professor G. H. DARWIN, F.R.S. Received November 8,—Read December 4, 1902.

(Abstract.)

'he first part of the paper deals with the vibrations and stability of ravitating elastic sphere. The matter is not necessarily homoeous, but is arranged in spherical layers. It is pointed out that, he classical investigation of the displacements produced in a graving sphere by given surface-forces, the most important of the gravional terms is omitted. The effect of this omission is to necessitate rrection, and this may entirely invalidate the solution when we are ling with spheres of the size of the earth or other planets. In fact, ppears that for a gravitating solid of the kind we are discussing the erical configuration may be one of *unstable equilibrium*, the instability ıg brought about by the gravitational terms in the manner already lained in a former paper.*

geneous, but in which the density is greatest near the centre, the values of ϕ will be greater than those just stated, but the critical vibration will still be such that the displacement is proportional to a first harmonic term.

In the former paper, already referred to, the suggestion was put forward that the instability of a nebular sun or planet, which, upon the nebular hypothesis, is supposed ultimately to result in the ejection of a satellite, may be largely brought about by a gravitational tendency to instability of the kind just described. We take, for the moment, an extreme hypothesis, and imagine that this agency is the preponderating agency, and that the rotational tendency to instability may be disregarded in comparison. We then find that when the ejection of a satellite is taking place, or has just been completed, the value of $\gamma\rho_0^2a^2/\lambda_0$ must be nearly equal to ϕ.

Except for the changes which have occurred since the consolidation of the planets, the solar system supplies material for testing this conclusion. When a number of planets of varying masses have thrown off satellites, we find (upon our present extreme hypothesis) that the masses ought to be proportional to the *squares* of the radii. It is found that this law is approximately obeyed in the solar system. It is further found that the absolute values of the masses and radii are approximately such as would be expected.

It is interesting to compare two extreme hypotheses, the first referring the phenomena of planetary evolution solely to rotational, the second solely to gravitational, instability. Given the approximate values of the density and elasticity of a planet, and the fact that this planet has thrown off a satellite; then the former hypothesis leads to a certain inference as to the angular momentum of the system, the latter to an inference as to the radius of the primary. The former leads to no inference at all as to the size of planets which are to be expected—they are as likely to be of the size of billiard-balls as of the size of the planets of our system—while the latter leads to no inference as to the angular momentum of the system, but presupposes it to be small. The contention of the present paper is that the inferences which are drawn from the former hypothesis are not borne out by observation on the planets of our system, while those which are drawn from the latter are borne out as closely as could be expected. The true hypothesis must of necessity lie somewhere between the two extremes which we are comparing, but the evidence seems to show that it is much nearer to the latter (gravitational) than to the former (rotational).

We next consider a number of questions connected with the figure of the earth. It seems to be almost certain that the present elastic constants of the earth are such that a state of spherical symmetry would be one of stable equilibrium. On the other hand, if we look kwards through the history of our planet, we probably come to a e when the rigidity was so small that the stable configuration of ilibrium would be unsymmetrical. At this time the earth would pear-shaped, and the transition to the present approximately erical form would take place through a series of ruptures. It is gested that the earth, in spite of this series of ruptures, still retains es of a pear-shaped configuration. Such a configuration would possess ngle axis of symmetry, and this, it is suggested, is an axis which ts the earth's surface somewhere in the neighbourhood of England possibly some hundreds of miles to the south-west of England). rting from England, we find that England is at the centre of a isphere which is practically all land: this would be the blunt end ur pear. Bounding the hemisphere we have a great circle of which land is the pole, and it is over this circle that earthquakes and canoes are of most frequent occurrence. Now, if we suppose our r contracting to a spherical shape, we notice that it would probably n the neighbourhood of its equator that the changes in curvature the relative displacements would be greatest, and hence we should ect to find earthquakes and volcanoes in greatest numbers near to circle. Passing still further from England we come to a great on of deep seas—the Pacific, South Atlantic, and Indian oceans:

species which are most nearly related to *B. mollis*, less and less readily species more remote from *B. mollis*, but in the same sub-genus, and least readily, or not at all, species in other sub-genera.

In the attempt to obtain some insight into what causes are at the bottom of this remarkable phenomenon—the predisposition to, or immunity from, infection of the host by the parasite—it was shown that anatomical differences on the part of the host-plants, such as the sizes and numbers of stomata, hairs, and so forth, do not suffice to explain it, since no relation could be detected between the curves expressing the percentage of infection and those expressing the sizes, numbers, &c., of the hairs, stomata, &c.

On the other hand, the evidence suggested some such assumption as the following. The fungus, when growing on a species of *Bromus* A, may refuse to infect another species B, either because B secretes some body of the nature of an enzyme or anti-toxin which effectually keeps the mycelium of the fungus at bay, or because the fungus habituated to the peculiar nutritive or other conditions afforded it by the host-plant A, cannot immediately adapt itself to the very different conditions offered by the species B.

Although the attempts to isolate any such anti-toxin failed, and experiments of a preliminary character to test the effect of differences of nutrition yielded little or nothing of a positive nature, I showed in the discussion of the probable factors concerned that some subtle relations between host and parasite must be assumed to account for the curious facts of immunity and predisposition on the part of the former, and of capacity and incapacity for infection on the part of the latter, in each case in various degrees according to the species of host offered for infection, or on which the fungus has hitherto been reared.

During the past year I have attempted to pursue this subject further, and limit myself for the present to the following theme. If the varying infective power of the fungus towards different species of host-plant is derived solely from the "nutritive conditions" afforded it by the host-plant it has hitherto been growing upon, two cases are possible—(1) these "nutritive conditions" may be simply the expression of the power of the tissues to yield certain food-substances to the parasite in proper proportions and in sufficient quantity, or (2) they may imply some more subtle relations between the mycelium of the fungus and the living contents of the host-cells. For instance, it may be not sufficient that the food-substances suitable to the fungus should exist in the cells of the host, but they must be there in a certain superabundance, or presented in a certain manner, and so on; or, it may be that the fungus must be vigorous up to a certain standard before it can obtain a hold on such food, and so on.

In order to test some of the possibilities here referred to I planned xperiments to see whether starving the host-plant of one or other f its necessary food-materials would (1) affect its predisposition to nfection, or (2) affect the capacity for infection of the fungus grown n the starved plants, or (3) in any other way affect the fungus or its ost.

On July 7, 1902, fourteen beakers were selected and filled with qual quantities of a clean coarse-grey sand, carefully washed and dried, ut still containing traces of ingredients which the root-hairs of such rasses as the Bromes are capable of selecting. This was not to be voided or regretted, since my object was not to attempt to grow seed-ings totally deprived of necessary salts, resulting in their premature eath, but to bring up plants so starved of certain such ingredients hat while they could go on living long enough for the purposes of he experiment, they would nevertheless exhibit the effects of the eficiency, and possibly re-act on the parasite.

The beakers of sand were lettered A to O, and treated as follows:—The beaker A received 200 c.c. of distilled water only, so that the nly mineral supplies available to the seedlings—after exhausting the races in the endosperm—would be such as the root-hairs could dis-olve from the sand grains. Another beaker received an equal uantity of a cold water-extract of fresh horse-dung representing a one, and the whole were now placed in a sheltered situation in the open, the bell-jars being raised on large blocks so that their rims just covered the rims of the beakers; thus permitting a free circulation of air, but protecting the whole from beating winds or rain. An awning was also provided, and carefully adjusted during hot days as necessary. Growth proceeded fairly rapidly, though of course some retardation was inevitable owing to the impervious nature of the beakers as contrasted with pots having porous sides, and by July 18 each beaker had from two to seven satisfactory seedlings.

On July 23, ten of the beakers had from five to seven excellent seedlings each: three had but four each, and one had only two successful seedlings. So far there was but little difference observable between the different seedlings, each of which was unfolding its third leaf. Nevertheless, there was evidence that the stores of food-materials in the indosperm were now being exhausted, and that the root-system was beginning to feel the effects of the differences in mineral supplies. Consequently, I fished out the remains of dead or un-germinated grains, and proceeded to infect certain of the seedlings in each beaker with Uredo-spores of *Puccinia dispersa* obtained from *Bromus squarrosus*. This species was selected because I had an abundant stock of vigorous spores at hand, and had already satisfied myself that they readily infect *Bromus secalinus*.

The infection was effected by placing equal doses of the fresh spores on each leaf chosen; the bell-jars were then lowered in order to keep the plants in a moist atmosphere for 24 hours, and then raised again on blocks for another 24 hours, after which the bell-jars were removed each morning, and only replaced at night or during rain.

The results of infection were already evident on one or two plants on July 31 and August 1, increasingly so on August 3, and by August 5 the pustules were as prominent as they ever became during the period of the experiment—*i.e.*, up to August 29—except in so far as some of the larger ones ran together or produced more spores.

The results are summarised in Table I, but it should be noted that it has been impossible to compress into the table the details of the observations from day to day as to the stature, robustness, colour, number and breadth of leaves and so on, or as to the rate of development of the plants and of the flecks and pustules of the parasite.

In order that the reader may gather an impression of the appearance of these seedlings during the progress of the experiment, however, I append photographs* of one set of the beakers in fig. 1, and of a representative series of the plants themselves, carefully extracted

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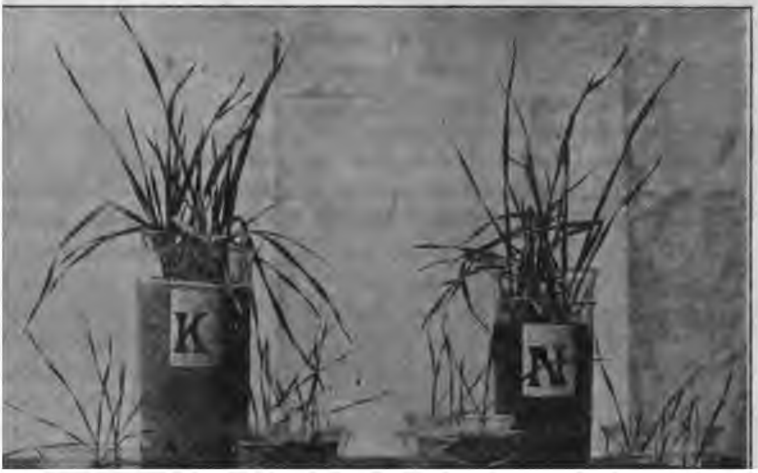
* I have to thank my son, F. K. Ward, for the preparation of these photographs.

m the loose sand in water and the roots washed and displayed on
ck paper (fig. 2); while in fig. 3 are appended other representative
ecimens, similarly carefully extracted and washed and displayed
th the roots on black paper, and the shoots on white paper.



psammophilous plants as the Bromes of the minute quantities of these elements needed for their growth; the root-hairs appear to be able to obtain traces of these substances from the grains of sand, and it would be necessary to repeat the experiments with some other medium—perhaps precipitated silica or pure quartz—to obtain through starva-

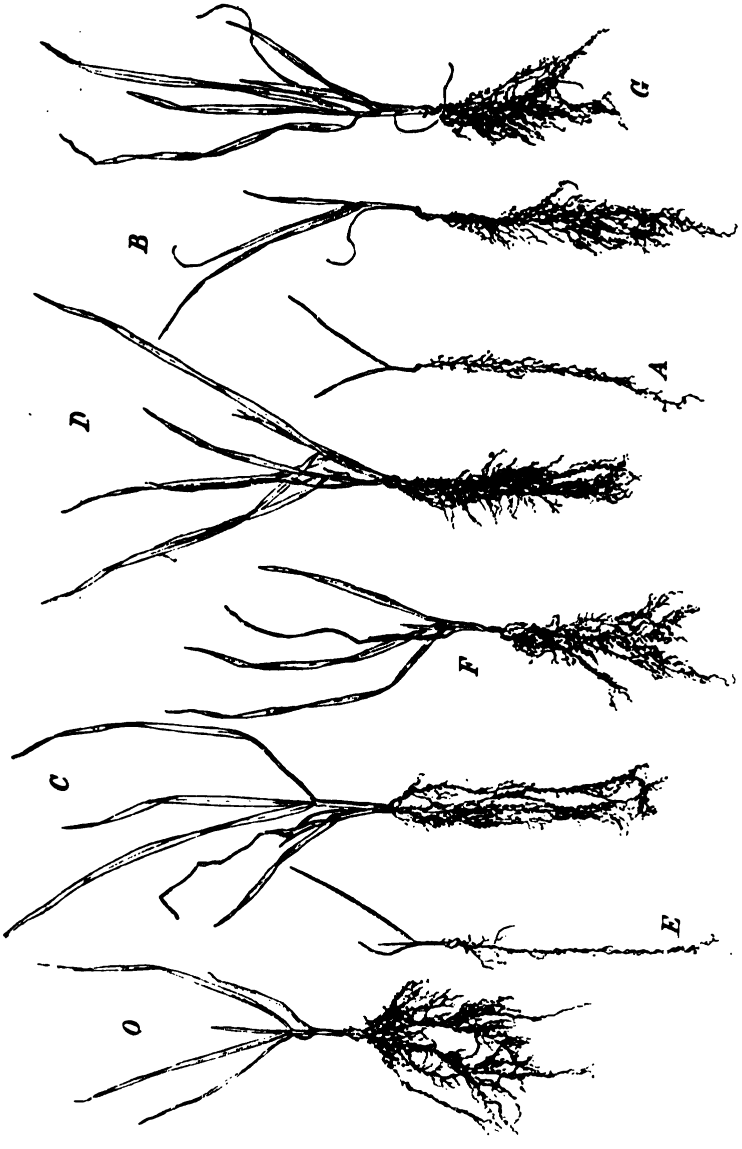
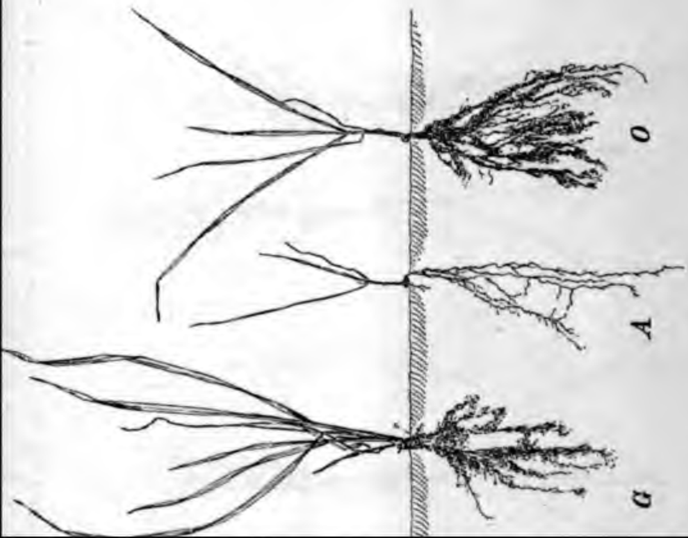


Fig. 2.—Plants of Experiment 47 removed from the sand and carefully washed, showing the development of root and shoot after 44 days. Treated as follows:—A distilled water; B minus phosphorus; C minus calcium; D minus magnesium; E minus nitrogen; F minus potassium; G normal solution; O horse-dung decoction.

ion of these elements, a result not wished for in the present connection.



sand, and then carefully washed and photographed with
; B minus phosphorus; D minus magnesium; G normal

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chemically purified by acid and washing, and stronger doses of the mineral solution were employed. Each beaker received an equal weight of the sand (1500 gr.), and of soluton (300 c.c.), and 8 grains of a species of *Bromus*, received under the name of *B. pendulinus*, were sown in each. This species—which differs little, if at all, from *B. patulus*—has been thoroughly investigated with regard to its predisposition to attack by the uredo of *Puccinia dispersa*, and was chosen because it has proved to be the most sensitive to infection of all the forms I have in culture. The grains were hand-picked and clean, but were not specially sterilised in any way.

The beakers were treated as before, and on July 15 seedlings were showing in all but one of the beakers, and by the 18th each of thirteen had from seven to eight seedlings, two had four each, and one none. By July 30 every beaker had from six to eight excellent seedlings, except one, and this remained barren to the end, possibly owing to drowning of the grains, or too deep sowing.

By this date there were marked differences in stature, and some in colour, between the various pairs of sets; the smaller seedlings in the beakers deprived of all added salts, or deficient in nitrogen or in phosphorus being particularly noticeable. Not only were the leaves ore, in Beakers. Sown Jul

Condition on August 8.		
umber fected st leaf.	Number infected on 2nd leaf.	Nur infe on 3r
0	0	
..	..	
3	5	
and 1 ecked	4 and 2 flecked	
0	1	? Fl
0	1	
1	3	1 ? fl
1	4	
0	4	
1	3	
0	3 flecked	
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Control cultures have shown over and over again that these seedlings do not incur the disease unless the spores are placed on the leaves, and I cannot accept these one or two apparent exceptions as evidence for a spontaneous outbreak of the disease.

The severity of the attack, as indicated by the size of the pustules and of the infected areas, and by the relative number of spores developed, followed much the same order as in the previous experiment, except that the nitrogen-free plants seemed to bear larger pustules than before.

The minute and poorly-developed pustules on the seedlings starved of phosphorus (*l* and *m*, Table III) or of all minerals (*a*) were particularly evident; as also were the severe attacks of the manured specimens. And, again, the magnesium-free seedlings showed premature yellowing and withering of the leaf-tips. Again, also, there was decided retardation of infection pustules in the phosphorus-free, distilled water, and, to a less extent the magnesium-free specimens.

But—again concordant—none of the plants were rendered immune from infection; and, as will be seen in the sequel, all the pustules, however minute, yielded spores with normal morphological characters, and perfectly capable of germinating and of reinfecting other seedlings of the *Bromus* in question.

Meanwhile, I had started a third complete series of sand-cultures of *Bromus pendulinus*, arranged as in the last series, but with two small variations in detail, viz., (1) in order to diminsh the risk of drowning or asphyxiating the seeds, I sunk a tall thistle-head funnel to the bottom of each beaker of sand, and poured the solution in each case down to the *bottom* of the beaker, allowing it to soak upwards and gradually moisten the upper layers from beneath; and (2) only 5 grains of the Brome (*B. pendulinus*) were sown in each beaker.

The results were very good. The grains were sown July 23. On July 29 seedlings were up in all but three of the beakers. On August 1 these also had seedlings showing, and by August 5 most of the seedlings were 25—30 mm. or more high.

On August 9 all but three of the beakers had four or five excellent seedlings, each with the second leaves showing, and the effects of the mineral starvation were beginning to be visible.

In order to emphasise this effect, if possible, by preventing the further development of the second and later leaves at the expense of materials stored in the *first* leaves, I now cut off the latter from each seedling with a pair of sharp fine scissors, and left the seedlings to grow further. During the next week the second leaf in each case attained its full size, and the third leaf began to appear; and I now proceeded to devote this series of seedlings to test the effect of sowing the spores reared on the seedlings of the last series (see Table III), and which had been affected as to their *numbers* by the starving or other treatment of the seedlings, on the leaves of the seedlings of this series, similarly starved or otherwise.

As we have seen (Experiment series No. 47, Table I, and series 60, Table II), spores from a seedling starved of phosphorus, however few in *number*, are perfectly capable of infecting the leaf of a *normal* seedling. Can such spores also infect another seedling *similarly starved of phosphorus*; and can spores reared on calcium-starved or nitrogen-starved seedlings, &c., infect seedlings *similarly starved of calcium or of nitrogen* respectively, and so on?

On August 18, therefore, I infected the third leaf of each of several seedlings in each beaker of series 55 (Table IV) with the spores developed on the corresponding seedlings (which had been similarly treated) of series 50 (Table III).

The results are summarised in Table IV.

As the table shows, practically all the infections were successful, showing that not only does mineral starvation *not prevent the development of virulent spores* on the seedling so starved—if the latter is inoculated with normal spores—but such starvation is also *incapable of incapacitating the corresponding seedling for infection* by means of spores grown on *similarly starved seedlings.*

We must therefore conclude for the present that (1) the starvation of mineral food-substances, although it reduces the size of the host-plant and seriously diminishes the *quantity* of spores which the mycelium can give rise to on its leaves, does not affect either the *virulence* of such spores or the *predisposition to infection* of the leaves of the Brome concerned.

Moreover (2), in view of the results with the highly-manured seedlings to which horse-dung decoction or normal mineral solution was added, it seems hopeless to expect that high cultivation of this kind will *diminish the predisposition of the plant to infection*—or, what would amount to the same thing in practice—*increase its resistance or confer immunity.*

The effects of manurial treatment are clearly quantitative only, so far as this question is concerned.

If the host-plant is highly fed, its tissues yield more food materials for the fungus; the latter can develope a larger mycelium, and produce a larger crop of spores. But so long as the host-plant is capable of living at all, it is a perfectly satisfactory prey for the fungus in its tissues, so far as *quality* of fungus food is concerned.

It seems to me that these results throw some new light on the problem of infection and parasitism, in so far as they bear out the view that the Uredine mycelium taxes the leaf—robs it of a share of its food-supplies—rather than destroys the protoplasmic machinery, at any rate during the vigorous period of growth and of production of Uredospores; and also in so far as they suggest that whatever may be the

August 18 with Spores of E

	Condition on Septe	
	No. of plates with pustules.	
....	3	Tw d
....	1	Ve
aves	4	Pat
....	5	Pat
....	4	Pat
....	5	Pat
....	4	Pat
....	4	Pat
....	1	La.
....	2	Pa
....	4	Tw
....	2	On
....	2	Ve
....	4	Pa
....	3	Pa
....	2	Ex

causes at work in the living cell which confer immunity or predisposition on the species of host-plant, or which confer virulence or impotence on the spore, they lie deeper than nutrition, reminding us once more of the significant resemblances which, as I pointed out in a previous paper,* exist between the phenomena of infection and those of pollination. It is in the highest degree improbable that the pollen-tube of a given species, A, is incapable of growth in the style and the ovule of an allied species, B, simply because the tissues of B do not contain suitable food-materials, while the pollen-tube of a species, C, readily fertilises a more distant species, D, simply because the latter does contain suitable nutritive materials, especially as in both cases we may be able to germinate such pollen in artificial sugar-solutions.

All the evidence points to the existence, in the cells of the fungus, of enzymes or toxins, or both, and in the cells of the host-plant of anti-toxins or similar substances, as the decisive factors in infection or immunity, although I have as yet failed to isolate any such bodies.

Moreover, I regard the results here given as furnishing strong evidence, on the whole, against any hypothesis which assumes the existence of a latent or lurking source of disease in the plants themselves, and as supporting the view that every patch of pustules originates from a definite infection spot due to the entrance of a germ-tube from a spore which has there germinated on the leaf.

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* 'Proc. Camb. Phil. Soc.,' vol. 11, part 5, p. 307.

An Experimental Determination of the Variation of the Critical Velocity of Water with Temperature."

By E. G. Coker, M.A. (Cantab.), D.Sc. (Edin.), Assistant Professor of Civil Engineering, and S. B. Clement, B.Sc., Demonstrator of Civil Engineering, both of McGill University, Montreal. Communicated by Professor Osborne Reynolds, F.R.S. Received July 16,—Read November 20, 1902.

(Abstract.)

The change from stream-line to eddy motion in water was first xamined by Osborne Reynolds, who in his earlier experiments, introluced colour-bands into a glass pipe in which water was flowing, to ndicate the change in the motion, and later observed the resistance ncountered in pipes over a great range of velocities.

The results of these experiments, and a consideration of the equations f motion, enabled him to express the laws relating to the critical elocity of water in pipes by the exceedingly simple equation

$$v_c = k\frac{\mu}{r\rho},$$

Numerous experiments at different temperatures were made when stream-line motion was maintained in the pipe, and the relation of velocity to slope of pressure was determined by logarithmic plotting, giving a series of lines, the "logarithmic homologues" at different temperatures. The positions of these lines were found to be in substantial agreement with those calculated from the equations of motion. Similar experiments for eddy motion were made and the logarithmic homologues were also plotted, and their intersections with the corresponding ones for stream-line motion determined. These intersections give the minimum critical velocity, and were found to lie very approximately on a straight line in the diagram.

The law of variation of critical velocity, v_c, with temperature was found to be

$$v_c^{-1} \propto 1 + 0{\cdot}03368\mathrm{T} + 0{\cdot}000156\mathrm{T}^2,$$

where T is the temperature Centigrade, which agrees very closely with the known variation in the viscosity of water, viz.,

$$\mu^{-1} \propto 1 + 0{\cdot}03368\mathrm{T} + 0{\cdot}000221\mathrm{T}^2;$$

and it may, therefore, be concluded that over the range of temperature examined the critical velocity of water in small pipes varies directly as the viscosity.

"Isomeric Change in Benzene Derivatives—The Interchange of Halogen and Hydroxyl in Benzenediazonium Hydroxides."

By K. J. P. Orton, Ph.D., M.A., St. John's College, Cambridge, Demonstrator of Chemistry, St. Bartholomew's Hospital. Communicated by Professor H. E. Armstrong, F.R.S.

Received December 1,—Read December 4, 1902.

In discussing the laws which govern substitution in the case of benzenoid compounds, Armstrong, in 1887, drew special attention to the peculiar behaviour of amido- and hydroxy-compounds, from which he inferred that the phenomena of substitution were less simple than was commonly supposed. He showed that there was evidence that the formation of para-derivatives was preceded by that of an isomeric compound formed by the displacement of the aminic hydrogen or hydroxylic hydrogen, and pointed to the probability that this might prove to be true also of ortho-compounds.

Since that time, it has been experimentally demonstrated by various chemists that the radicles, Cl, Br, I, NO_2, SO_3H, can all be introduced in place of the hydrogen of the amino-group of anilines and of the imino-group of anilides, and that the compounds thus formed can be ianged into isomeric substances in which the substituent is contained the hydrocarbon nucleus.

The transformation of the aniline derivative appears to take place ıly in the presence of some other substance: in the case of the henylacylchloramines, such as $C_6H_5.NCl.COCH_3$, for example, appantly the change into the isomeric chloroacetanilides takes place only nder the specific influence of hydrogen chloride.* The investigation such cases of isomeric change is, in fact, of special interest as they e, so to speak, "fermentative" in character, often taking place with markable facility, and under the influence of minute amounts of e substance, which apparently provokes the change—the catalyst. easurements of the velocity at which changes of this type occur, for ample, of diazoaminobenzene into aminoazobenzene,† show that a locity-coefficient of constant value is given by the equation,

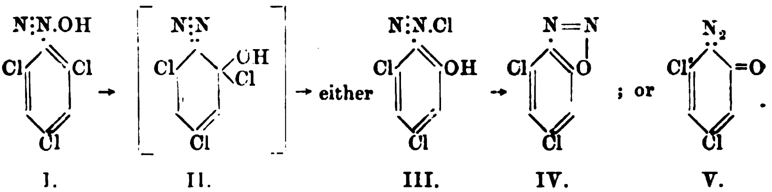
$$k = t^{-1} \log a/(a-x).$$

he isomeric change is, therefore, apparently a so-called monoolecular change; but, as in other cases, the slowest of the series of mple changes which make up the complete transformation is alone easured; moreover, the substance which conditions the change is not ken into account.

less, the diazonium salt being immediately converted into the alkali diazotate.* The latter change may be represented thus—

$$\underset{\text{Diazonium salt.}}{C_6H_2X_3.NR{:}N} \rightarrow \underset{\text{Diazonium hydroxide.}}{C_6H_2X_3.N(OH){:}N} \rightarrow \underset{\text{Diazotate.}}{C_6H_2X_3.N{:}N{\cdot}OM'}.$$

As the author has maintained on previous occasions, the first step in this type of intramolecular change consists in a transference of the atom or group attached to the nitrogen to the ortho- or para-carbon atom of the nucleus, an ortho- or para-quinone being formed. The complete transformation in the case here described may be represented in the following manner:—



In other words, the diazonium chloride (III) may be represented as changing into the "diazophenol" (IV); or, if, following Wolff† and Hantzsch,‡ the diazophenols are considered to be diazoquinones (V), the ortho-quinone form (II) merely loses hydrogen chloride.

Closely allied to the intramolecular change just mentioned, are Hantzsch's observations§ that *s*-tribromobenzenediazonium chloride changes into chlorodibromobenzenediazonium bromide, and chloro- and bromo-benzenediazonium thiocyanates into thiocyanobenzenediazonium chlorides and bromides.

[The following observations of Meldola‖ may also be mentioned, as the author's results offer a possible explanation. When dinitro-*o*- or dinitro-*p*-anisidine is treated with sodium nitrite in the presence of acetic acid, the nitro-group occupying a position ortho or para with respect to the amino-group is eliminated, a diazophenol (diazo-

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* Professor Meldola has called the author's attention to a French patent (No. 315,932) of the Badische Aniline Company, dated February 28, 1902, in which the replacement of the nitro-group and halogen by the hydroxyl-group *by the action of alkalis* on diazonium salts, is claimed as a technical process. The author's experiments show within what limits this process is applicable, and what is the probable action of the alkali.

† 'Annalen,' 1900, vol. 312, p. 119.

‡ 'Berichte,' 1902, vol. 35, p. 888.

§ 'Berichte,' 1896, vol. 29, p. 947; 1897, vol. 30, p. 2334; 1898, vol. 31, p. 1253; 1900, vol. 33, p. 505.

‖ 'Trans. Chem. Soc.,' 1900, vol. 77, p. 1172; 'Proc. Chem. Soc.,' 1901, vol. 17, p. 131; 'Trans. Chem. Soc.,' 1901, vol. 79, p. 1076; 1902, vol. 81, p. 988.

ide) being formed. In diazotising in the presence of hydrogen
loride, an ortho-nitro group is replaced by chlorine.]

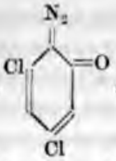
The isomeric changes of the diazonium salts differ, however, some-
hat from the similar changes of the phenylacylchloramines, phenyl-
ramines, &c. The latter compounds are *per se* relatively stable,
d apparently only undergo change in the presence of some agent
talyst); the diazonium compounds, on the other hand, appear to be
rinsically labile, and in aqueous solution, at least, capable of passing
o a more stable configuration. In the latter case the nitrogen atom,
aring the wandering group, the hydroxyl group (or, as in Hantzsch's
tances, the chlorine atom or the thiocyanate group), is pentad,
us, Ph.N(OH):N, whilst in the phenylacylchloramines, &c., it is
ad. The idea at once suggests itself that in the case of the last-
ntioned compounds, the first action of the catylist is to form an
ditive product, in which the nitrogen is pentad. The product, in
ich, it should be noted, more than one negative radicle is attached
the nitrogen, is not a stable compound, and is now capable of
ssing into the isomeric quinone form, and thus start the transforma-
n.

These results are also of interest, inasmuch as Hantzsch* has
ted recently that he has obtained *s*-tribromophenylnitrosamine,

star-shaped aggregates, which are very soluble in water (SO_4 found 31·0, calculated 31·4 per cent.). s-*Trichlorobenzenediazonium nitrate*, $C_6H_2Cl_3\cdot N(NO_3):N$, prepared in a similar manner, forms colourless needles. When kept for two or three days, both salts begin to show signs of change, which are more marked in the case of the nitrate. The latter becomes noticeably yellow, and when dissolved in water forms a yellow solution. The initially colourless aqueous solution of the pure colourless salts becomes yellow after a few hours; after 24 hours chloride is just recognisable in the solution. In one experiment, 0·5 gramme of the acid sulphate was dissolved in 100 c.c. of water; after 3 days a very small amount of a yellowish-red solid had separated from the yellow solution; at the end of 16 days, the chloride in solution was precipitated by silver nitrate in the presence of nitric acid: the silver chloride weighed 0·05 gramme, whereas for the complete conversion of the diazonium salt into the diazophenol, 0·235 gramme of silver chloride should have been found.

An aqueous solution of *s*-trichlorobenzenediazonium acetate, obtained by mixing neutral solutions of the diazonium nitrate and sodium acetate (molecular proportions) rapidly becomes yellow and acid. In a short time the solution becomes turbid, and after 4—5 hours deposits a bright yellow amorphous solid. In an experiment, in which 1·75 gramme of the diazonium nitrate, dissolved in 150 c.c. of water, was treated with sodium acetate (1 mol.), and kept at 10—15° for 40 hours in the dark, an estimation of the hydrogen chloride in the solution showed that 54·5 per cent. of the diazonium compound had changed into the diazophenol.

When instead of sodium acetate, sodium hydrogen carbonate was used a similar change took place, but far more rapidly. In one experiment a dilute aqueous solution of sodium hydrogen carbonate (3 mol.) was added drop by drop during a period of 1 hour to a cooled solution of 1 gramme of the diazonium hydrogen sulphate. (The bicarbonate was finally present in sufficient quantity to combine with the whole of the sulphuric acid and one-third of the chlorine present in the diazonium salt.) After one-third of the bicarbonate had been added, and the acid converted into the normal sulphate, the solution rapidly became yellow and deposited a yellow solid. Throughout the experiment the mixture was acid. As soon as the whole of the bicarbonate was added, the chlorine was estimated; it represented 54·5 per cent. of the amount which should be obtained were 1 atom of chlorine eliminated from the diazonium salt. In another experiment, using the same quantities of diazonium salt and sodium bicarbonate, the solutions were mixed as rapidly as possible; a copious yellow precipitate at once appeared; the chloride in the filtrate represented 72 per cent. of 1 atomic proportion of chlorine.

3 : 5-*Dichloro-o-diozophenol* (3 : 5-*dichloro-o-diazoquinone*),  , is

ntained in the yellow solutions which are obtained by any of the ethods just described. It is best prepared by adding excess of dium acetate to *s*-trichlorobenzenediazonium hydrogen sulphate or trate. The mixture should be kept during 40—50 hours in the rk ;—in the light the solution darkens, the diazophenol decomposing. he liquid was then filtered from the amorphous yellow solid, made rongly acid with nitric acid, and extracted four or five times with her. On evaporating the yellowish-brown extract, a mixture of l and crystals remained. In order to obtain the diazophenol, dry ydrogen chloride was passed into the ethereal extract ; this caused e *hydrochloride* of the dichloro-*o*-diazophenol, $O{:}C_6H_2Cl_2{:}N_2,HCl$, to parate in small needles. This salt was converted into the diazo-henol by treatment with a small quantity of water. Thus prepared, e diazophenol is an orange powder, which crystallises from an hereal solution in flattened orange prisms, which melt at 83—84°, rming a red liquid ; at 87° the latter decomposes. On analysis, cannot be got to crystallise from any solvent; they decompose at 85°, and in a moist atmosphere slowly give off nitrogen; this is very noticeable when an attempt is made to estimate the nitrogen by Dumas' method. They do not dissolve in aqueous alkalis or acids. Cryoscopic determinations of the molecular weight in benzene show that, whether prepared by means of sodium acetate or bicarbonate, the molecule contains three benzeneazo-groups. There are three such compounds possible, formed by condensation either of (1) 2 mols. of *s*-trichlorobenzenediazonium salt with 1 mol. of the diazophenol ($(C_6H_2Cl_3{\cdot}N_2)_2{\cdot}C_6Cl_2{<}^{N_2}_{O}$ has a mol. weight = 604; chloride = 46·8 per cent.); or (2) of 1 mol. of the diazonium salt with 2 mols. of the diazophenol (mol. weight = 585·5; chlorine = 42·2 per cent.); or (3) of 3 mols. of the diazophenol (mol. weight = 567; chlorine = 37·5 per cent.). The material prepared by the use of sodium acetate has the highest percentage of chlorine, 44·7—44·9; its molecular weight was found to be 580—590; it is probably a mixture of (1) and (2). When bicarbonate is used, the yellow substance contains 41·4—43·7 per cent. of chlorine; its molecular weight was found to be 560—580.

The Transformation of s-*Tribromobenzenediazonium Hydroxide.*

The transformation of *s*-tribromobenzenediazonium hydroxide resembles in its main features that of the *s*-trichloro-compound. *s*-Tribromobenzenediazonium nitrate, when first prepared, is pure white, although Silberstein* has described it as yellow. On keeping, it gradually acquires a yellow colour. Solutions both of the nitrate and the hydrogen sulphate become yellow, and bromine can be detected in the liquid. When either sodium hydrogen carbonate or sodium acetate is added to solutions of the salts, the decomposition is far more rapid. Thus in one experiment, in which sodium bicarbonate was added to a solution containing 1 gramme of the diazonium hydrogen sulphate, 0·31 gramme of silver bromide was obtained, whereas 0·508 gramme is required for the elimination of one atom of bromine.

In the presence of sodium acetate, the solutions of the diazonium salt rapidly become yellow and turbid. After 24 hours at 0°, a bulky and, at first glance, homogeneous precipitate had separated. In one experiment when 2 grammes of the sulphate were used, 0·58 gramme of silver bromide was obtained from the filtrate from the yellow precipitate. Careful examination of the yellow precipitate showed that it consisted of crystals completely covered and matted together by an amorphous yellow powder. These two substances could not be separated by crystallisation from any solvent. It was, however, found possible obtain the crystals free from the powder by frequently shaking up he solid with water and decanting the supernatant liquor before the ght powder had had time to settle.

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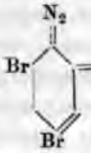
* 'Journ. Prakt. Chem.,' 1883, [2], vol. 27, p. 113.

3 : 5-*Dibromo-o-diazophenol* (3 : 5-*dibromo-o-diazoquinone*),  .—

he crystals, just mentioned, are the 3 : 5-dibromo-*o*-diazophenol in a ate of purity. They are long slender transparent prisms of a fine range colour, which explode when heated at 140°.

0·1894 gramme gave 0·2564 gramme AgBr. Br = 57·6.

Two cryoscopic determinations of the molecular weight in benzene olution gave the values 278·9 and 281·6.

$C_6H_2ON_2Br_2$ requires Br = 57·53 per cent., and a molecular weight f 278.

The compound is very soluble in chloroform, benzene, ether and lacial acetic acid, and in boiling alcohol. It is moderately soluble in oiling water, and very slightly so in cold water or petroleum. Treatment with hot water or alcohol decomposes it. It dissolves in conentrated acids, forming a colourless solution, and is reprecipitated by of the powder, a small quantity of the hydrochloride of the last-mentioned substance separated.

[The investigation of these intramolecular changes is being continued. It has been found that a similar interchange of halogen for hydroxyl takes places very readily in solutions of chloro- and bromo-naphthalenediazonium salts, even in the presence of excess of acid. The author is of opinion that the observation of Gaess and Ammelburg,* that an aqueous solution of 1-nitro-2-naphthalenediazonium sulphate yields the 1 : 2-naphthalenediazo-oxide, is an example of the type of transformation here considered.]

"On certain Properties of the Alloys of the Gold-Silver Series." By the late Sir W. C. Roberts-Austen, K.C.B., D.C.L., F.R.S., and T. Kirke Rose, D.Sc. Received October 22,—Read December 11, 1902.

[Plate 3.]

In a former communication to the Society† the curve of the initial freezing points of the alloys of gold and copper and some micrographic evidence as to their structure were given, and it was shown that according to the theory of solutions the alloys rich in gold should not be homogeneous after they have solidified. The fact that they are not uniform was confirmed by analysis. The subject has, however, more than theoretical interest, and the inference was drawn that standard gold, which consists of eleven parts by weight of gold to one of copper, is unsuitable as a material for the preparation of the trial plates by which the standard of the coinage is tested. These trial plates according to law must contain $916 \cdot \dot{6}$ parts of gold and $83 \cdot \dot{3}$ of "alloy," that is of some other metal, and it remained to be determined what the other metal should be.

It will be at once apparent that the alloy or mixture of the two metals must, if the cold mass is to be uniform, solidify as a whole, that is to say, that the crystals first formed should be of the same composition as the mother liquor, and this condition can be fulfilled by isomorphous mixtures only. It has long been recognised that the gold-silver alloys are cases of isomorphism, and Gautier, in 1896, stated‡ that the freezing-point curve of the series followed a straight line if the percentages by weight of the constituents were taken as abscissæ.

This curve was re-determined by experiment, a number of alloys being made up and autographic records taken of their cooling curves by the Roberts-Austen recording pyrometer. The results obtained are given in the following table, and have been plotted in fig. 1, in which the abscissæ are atomic proportions of the metals in the alloys. The freezing point of gold was taken as 1064°.

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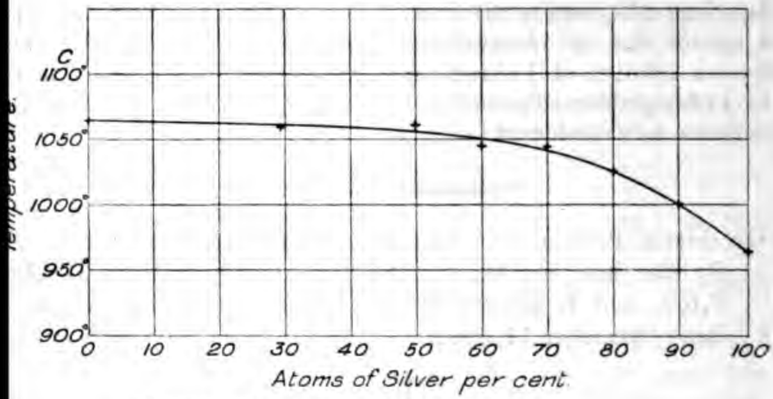
* 'Berichte,' 1894, vol. 27, p. 2211.

† 'Roy. Soc. Proc.,' vol. 67 (1900), p. 105.

‡ 'Bull. de la Soc. d'Encouragement,' Oct., 1896.

Fig. 1.



Percentage of gold present in alloy.		
By weight.	In atoms.	Freezing point.
100	100	1064°

FIG. 2.

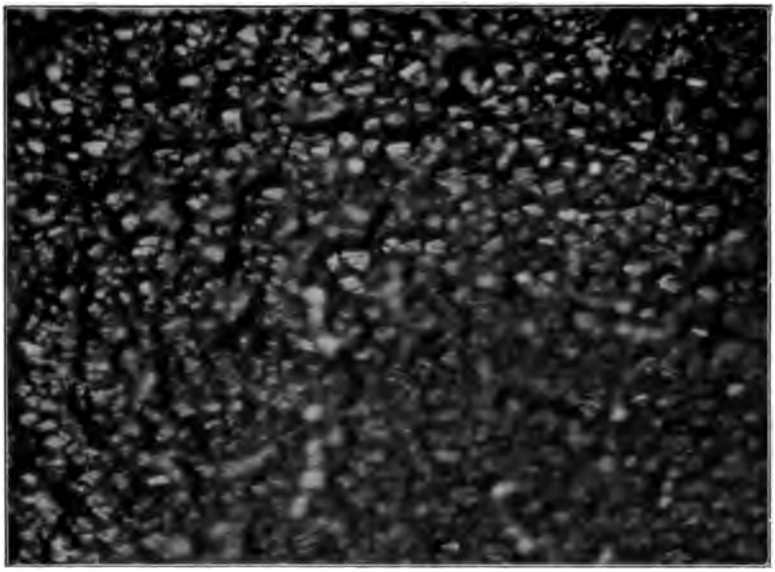
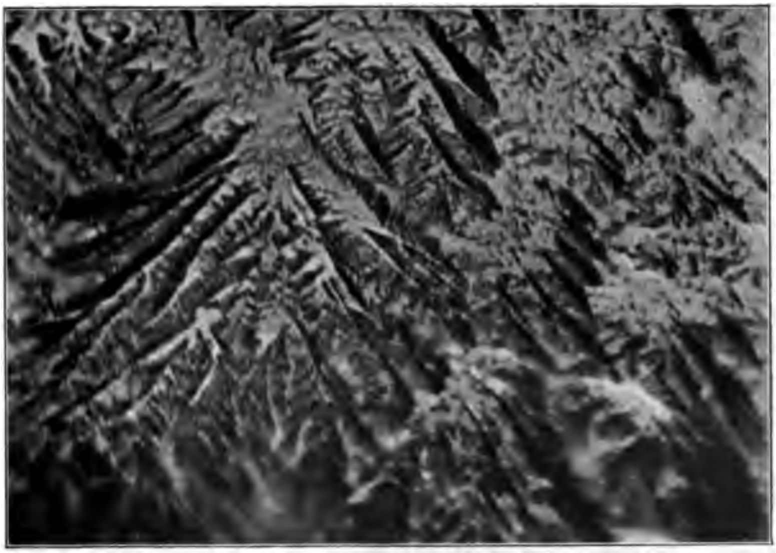


FIG. 3.



There is, of course, no eutectic alloy observable in any member of the series.

The alloys all consist of large grains, but these are built up of smaller grains, so that the ultimate structure is exceedingly minute. When magnified 1500 diameters the grains appear as small irregular crystals of the cubic system (see fig. 2, Pl. 3). In order to develop any segregation that might take place, an ingot of the standard alloy containing 91·66 per cent. of gold by weight was heated for 2 months in one of the annealing furnaces at the Royal Mint, the temperature of which was kept at about 700° by day, but fell to about 100° at night. The maximum temperature attained was over 300° below the fusing point of the alloy, and the sharpness of the angles of the specimen had suffered no change. After this treatment it was found that the grains had increased in size, and the crystals forming them had become well developed, as shown in fig. 3 (Pl. 3), in which the structure is magnified 1500 diameters. No true segregation, however, could be detected even in this ingot, either by analysis or by the microscope, and plates prepared by rolling out ingots containing $916\cdot\dot{6}$ parts by weight of gold, and $83\cdot\dot{3}$ parts of silver, were found on analysis to be uniform in composition.

The ancient trial plates, according to the analysis made by one of us,* consist of a triple alloy of gold, silver, and copper. The earliest one in existence was made in 1527, the year following the first introduction of the standard $916\cdot\dot{6}$. This plate contained only 0·62 per cent. of copper, and was probably intended to consist of gold and silver only. All subsequent plates, however, down to that made in 1829, contained much larger amounts of copper. In 1873 it was determined to omit the silver and to use only copper as the alloying metal, and thus to preserve identity of composition between the trial plate made in that year and the coinage. In view, however, of the importance of obtaining homogeneous trial plates and of the ease with which the exact quantity of copper required to make the assay pieces identical in composition can be added to the pieces of the trial plate during the course of the assays, it is preferable to use only silver as the alloying metal in the manufacture of the trial plates.

Such an alloy has accordingly been used at the Royal Mint since the beginning of the present year instead of fine gold for checks in the assay of standard bars and coins. In view of the minute accuracy with which the operations of coinage have to be conducted, this is a matter of much importance. By this method any errors are avoided which might be caused by accidental variations in weights occurring after the trial plates have been made.

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* Roberts-Austen, 'Chem. Soc. Journ.,' 1874, p. 197.

"Abnormal Changes in some Lines in the Spectrum of Lithium."

By Hugh Ramage, B.A., St. John's College, Cambridge. Communicated by Professor G. D. Liveing, F.R.S. Received November 19,—Read December 11, 1902.

In the course of an investigation on the flame spectra of metals, the writer has examined the spectrum of lithium. Some facts have been discovered of sufficient importance for a separate paper.

The same spectrometer was used as in the investigation on the "Spectra of Potassium, &c."* Whilst the wave-lengths of some of the lines in the flame spectrum of lithium agree closely with those given by Kayser and Runge for the lines in the arc spectrum,† the wave-lengths of other lines differ considerably from these. The numbers and differences are given in the following table:—

Oxyhydrogen flame spectrum (Author).	Arc spectrum (Kayser and Runge).		
	Principal series.	Second subordinate series.	First subordinate series.

The only important differences occur in the lines of the first subordinate series. Exner and Haschek have given the wave-lengths of three lines in the spark spectrum[*] as 2815·55, 3232·91, and 4603·10. The last is described as reversed. Eder and Valenta have given the wave-length of the blue line in the Bunsen flame spectrum as 4602·4,[†] and as 4602·46 in the spark spectrum when a condenser was used.[‡] They described the latter measurement as that of the middle of a broad dark line, the less refrangible wing of which was stronger than the other, and diffuse towards the red. They also gave measurements of five other lines, and these agree closely with the arc lines given by Kayser and Runge. Professor Hartley recorded four lines in the oxyhydrogen flame spectrum of lithium chloride[§] corresponding to the first, fourth, sixth and eleventh of the lines in the above table. The blue line was measured in the flame spectrum on four plates, and the results differed only in the second decimal place, the figures in which were 8, 7, 6 and 7. Professors Liveing and Dewar made some observations by eye on the appearance of the blue line in the arc spectrum which led them to believe there were two lines, a strong one with a weak line on the more refrangible side,[||] and Kayser and Runge, after referring to this, say:—[¶] " Wir haben nur bei zwei Aufnahmen neben der Hauptlinie eine zweite schwache umgekehrte Linie bei 4603·13 erhalten; da aber hier eine Eisenlinie liegt, glauben wir, dass dies eben die Eisenlinie ist, welche sich durch den hellen Hintergrund der Lithiumlinie umgekehrt hat."

The wave-length of the line which Kayser and Runge attributed to iron agrees with that obtained by the author for the bright line in the flame spectrum, and by Exner and Haschek for the reversed line in a spark spectrum. It was decided in view of these differences to take a series of photographs of the arc spectrum of lithium, using the carbonate of lithium on carbon poles, and to study especially the appearances of the blue line. The results will now be briefly described.

A Gulcher arc lamp with vertical carbons was used. When the arc was started with a quantity of lithium carbonate on the carbons, a large proportion of the salt was freely volatilised and expelled in the form of a dense vapour. As the arc lamp was placed, the magnetic field produced by the feed mechanism caused the bulk of the vapour to be expelled in the direction of the collimator. Photographs of the spectrum taken at this early stage show the line as a very broad bright line extending from 4610·4 to 4593·5, with a narrow dark line extend-ng (2 in figure) from 4603·46 to 4602·71, having its middle (measured) t 4603·06. The middle of the broad bright line is, according to the bove, at wave-length 4601·95, and the middle of the dark line (reversed) s coincident with the bright line of the flame spectrum.

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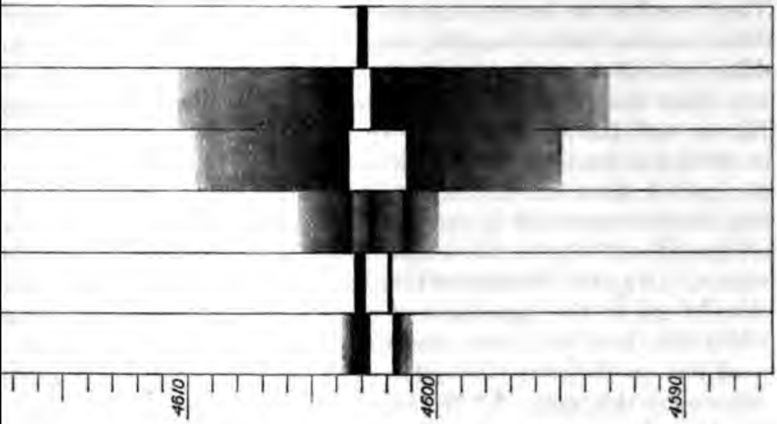
[*] 'Sitzber. kais. Akad. Wien,' vol. 106, Abth. 2A (1897), p. 1138.
[†] 'Denkschr. kais. Akad. Wien,' vol. 60 (1893).
[‡] 'Denkschr. kais. Akad. Wien,' vol. 67 (1898).
[§] 'Phil. Trans.,' A, vol. 185 (1894), p. 177.
[||] 'Phil. Trans.,' vol. 174 (1883), p. 215.
[¶] *Ibid.*, p. 20.



Some photographs were also taken of the spectrum of the transparent flame which issued from the arc, the bar magnet being used as above. With a short exposure one narrow bright line was obtained of wave-length 4603·06; with a longer exposure this bright line was much stronger, and there was the appearance of a weak line on its more refrangible side, exactly as Professors Liveing and Dewar described. The wave-lengths of these two were 4603·18 and 4601·89 respectively on one photograph (5 in figure), and 4603·12 and 4601·65 on another. The last line was fairly sharp on the less refrangible side, whilst it faded gradually on the other side (6 in figure). With still longer exposures the weaker line could not be distinguished from the wing of the much expanded bright line. This wing when weak always resembles a line, and it was thought that a line might be nearly coincident with its less refrangible edge. Measurements showed that this edge varied in position on different photographs, but Professor Liveing suggested that the tip might be formed by a line, and if so, the tip should always have the same wave-length. The following table gives the wave-lengths of the two tips on different photographs:—

Spectrum.	Tip of weaker side.	Tip of stronger side.
206^3	4601·68	
213^4	·83	4603·11
213^6	·66	·11
214^1	·72	·03
222^6	·78	·10
222^7	·82	·11
224^6	·82	
226^5	·83	
226^7	·95	4603·21
232^6	·79	

This evidence is not conclusive; both tips appear to vary, but the weaker one varies more than the other. The arc, in these experiments, was formed with a Gulcher lamp with the carbons vertical, the positive being uppermost, and the image of the arc projected on to the slit of the collimator by a lens. It was observed that the less refrangible point, wave-length 4603·1, was, in many photographs, higher on the plate than the weaker point; it was given out by vapour quite near to the positive pole. It was observed also in some photographs that the line extending from it faded away lower down; the vapour near the negative pole did not emit this line but gave the broad bright line with the broad dark line down its middle. This broad bright line fades away as the positive pole is approached; the more refrangible wing ends in a point, but the less efrangible wing is lost in the bright line of wave-length 4603·1. here appears then in the middle portion of the spectrum a broad ark line with wings of unequal extent, and the less refrangible wing s broader, and, near its more refrangible edge, much stronger than he other wing. The broad dark line extended on such photographs rom (1) 4602·72 to 4601·81, (2) 4602·86 to 4602·05, (3) 4602·73 to 601·62, and in two other cases, where there were less differences of ntensity between the two wings, from 4603·26 to 4601·61 and from 603·07 to 4601·59. There were indications on some photographs of bright line near the middle of the broad dark line; the measurenents of such a photograph gave :—

Less refrangible edge of broad dark line	4603·40
Apparent bright line	4602·57
More refrangible edge of broad dark line......	4601·31

The absorption bands of reversals in such a photograph as this re comparatively bright, for there is considerable action on the plate here the images of these bands fall (4 in figure). The effect is probbly due tothe superposition of the spectrum of the outer flame upon hat of the inner core when both are giving reversed lines.

Some observations were made at Professor Liveing's suggestion on he arc spectrum of lithium when the arc was formed between carbon moist lithium carbonate which had been fused into the aluminium cup. The blue line was very weak and nebulous, and it was difficult to obtain measurements of it. The following were obtained from four photographs :—4603·18, 4603·14, 4602·99 and 4603·97; the mean of these is 4603·15. Better photographs were obtained when a coil of wire was also introduced into the secondary circuit. The line more nearly resembled that obtained when no Leyden jar was used; the broad reversed line was more clearly defined at the electrode when negative than when positive.

An attempt was made to photograph the blue line in the Bunsen flame spectrum, but with an exposure of six hours no trace of it was obtained.

Other lines in the Spectrum of Lithium.

The orange line was examined in the arc and flame spectra by eye observations and by photography. The line in the flame spectrum was measured on four plates :—

Spectrum.	Wave-length.	
244^2	6103·84	centre of line.
244^2	6103·85	near tip of line.
244^4	6103·88	rather strong line.
245^2	6103·84	
245^3	6103·83	

The mean of these, omitting the rather strong line, is 6103·84. None of these lines showed any signs of reversal.

The wave-length of the bright line in the arc produced by the Gulcher lamp in the open was :—

Spectrum.	Wave-length.	
241^6	6103·81	nebulous in middle with sharp points. The nebulous part extended from 6102·46 to 6104·94.
242^4	6103·86	
242^5	6103·82	

The mean result is 6103·83.

The orange line is easily reversed in the arc. The reversed line was photographed and the wave-length measured on two plates, the results being 6103·82 and 6103·84. The reversal was narrow and the wings did not extend very far. The more refrangible wing was slightly broader than the other, and this difference was observed in the photographs and by eye observations. The part of the arc near the negative pole was examined very carefully; the line was most sharply reversed in that part, but there was no trace of a broad dark line. The orange line, with the exceptions noted, behaved normally.

Measurements were also made of two other lines in the arc spec-rum. The wave-length of a weak and very diffuse line on one plate ras 4132·82, and that of a very weak sharp line, 4273·32. The ormer line on another plate was stronger and broader; its middle vas at 4132·35, whilst the other line, much broadened towards the ed, had its strongest part at 4273·62.

The current used in working both the open and enclosed arcs was bout 9 ampères. Kayser and Runge employed a current of 25—35 mpères from one machine and, to bring out the weak lines in some pectra, a current of 40—50 ampères from another machine. It seems robable that they worked with the intense arc which was only obtained y the author when the carbons were very near together, and that they bserved only the spectrum of broadened lines which the author found vas emitted by the intense arc and near the negative pole by weaker rcs. Their remarks on the appearances of the lines in the first and econd subordinate series confirm this view. Eder and Valenta btained a similar spectrum to Kayser and Runge's arc spectrum in he spark spectrum of metallic lithium when they used a condenser in he secondary circuit.

The author has pleasure in thanking Professor Liveing for the nterest he has taken in this work and for some useful suggestions.

[ADDED NOV. 28, 1902.—Since this paper was communicated to the Royal Society, I have seen a paper on the spectrum of lithium, by Hagenbach, in the 'Annalen der Physik,' No. 12, 1902, which was published on November 13. The experimental part of his paper deals almost entirely with the blue line, and the fact that there are other abnormal lines in the spectrum of lithium is recorded above for the first time. Hagenbach's conclusion that there are two lines near wavelength 4603 is not, I think, established; and I still hold that the views expressed in this paper are more probable. He has not been able to find the second line as a bright line, so the difficulties in the way of accepting the view that there is a second dark line, without a corresponding bright line, remain. He has not referred to Professors Liveing and Dewar's work,* and his evidence for saying there are two lines is, in fact, similar to the evidence they gave.]

'An Error in the Estimation of the Specific Gravity of the Blood by Hammerschlag's Method, when employed in connection with Hydrometers." By A. G. LEVY, M.D. (London). Communicated by Sir VICTOR HORSLEY, F.R.S. Received November 25,—Read December 11, 1902.

(From the Laboratory of Pathological Chemistry, University College, London.)

Hammerschlag's method of estimation of the specific gravity of the blood is an application to clinical purposes of a physical method frequently employed when only a small quantity of the substance under investigation is obtainable. The method may be briefly described as the adjustment of the specific gravity of a mixture of chloroform and benzol by small successive additions of either constituent until it corresponds to the specific gravity of the blood, the test of the attainment of this condition being that a small drop of the blood, when immersed in the mixture, shall remain suspended without any very obvious tendency to rise or sink. The specific gravity of the mixture is then estimated by means of a hydrometer, the scale of which is graduated to register densities lying between the maximum and minimum densities of blood, *i.e.*, from 1·020 to 1·080.

In order to attain a rapid adjustment of the relative proportions of the chloroform and benzol, it is the general practice to use a comparatively small quantity only of these fluids and a small hydrometer, and, as will be hereafter seen, the size of the instrument is an important factor in the magnitude of the error.

This error was commented upon in a paper read by Dr. Baumann efore a recent meeting of the Physiological Society, in which he ecorded estimations by Hammerschlag's method which exceeded by ·012 the estimation of the specific gravity of the blood by the icnometer method. Dr. Baumann also mentioned similar, but less onsiderable, excessive readings noted by certain other observers. I ad myself, in the course of a series of experiments upon the blood f dogs,* occasion to remark upon the consistently high results ielded by Hammerschlag's method, the excess being, in my cases, rom 0·007 to 0·008.

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* 'Phil. Trans.,' vol. 174 (1883), p. 215.

With the purpose of investigating the source of this error, I repared two mixtures, the one (A) of chloroform and benzol, and he other (B) of glycerine and water, and, in each case, adjusted he relative proportions until identical readings were obtained on he scale of the same hydrometer. On immersing a drop of mixture B) in a vessel of mixture (A), it rapidly sank to the bottom, thus ndicating, in the absence of interfluid exchanges, an actually higher pecific gravity of (B). This indeed could be demonstrated by other ethods of finding the densities of the two liquids, *i.e.*, by the weighng bottle or picnometer, or by Westphal's specific gravity balance. he occurrence of a gross fault in the hydrometer method being stablished, it remained to investigate its extent and origin.

Table I.

No. of hydrometer	No. 1.	No. 2.	No. 3.	No. 4.
Weight in grammes	15·3118	11·367	3·81	2·868
Diameter of stem in mm.........	3·95	4·37	3·55	3·13
Length of first division of scale (*i.e.*, 1·000 to 1·001) in mm.	1·29	0·74	0·417	0·398
Reading of scale in a chloroform benzol mixture of specific gravity = 1·000	1·002	1·003	1·0095	1·010

No. 1 hydrometer is a more sensitive instrument than the ordinary urinometer, having a large barrel and a comparatively fine stem. No. 2 is an ordinary urinometer such as is in general use in hospitals. Hydrometers Nos. 3 and 4 are considerably smaller instruments, and are similar to those which have been employed in this laboratory for use in connection with Hammerschlag's method.

immersion which corresponds to the mark 1·000 on the stem when the hydrometer is floating in water.

If the same hydrometer is floated in a chloroform and benzol mixture of sp. gr. = 1·000, the same volume of mixture is displaced, but, as in this case the liquid possesses a lower surface tension, the pull upon the stem is less powerful, and hence less of it is immersed from this cause than in the case of water, the degree of surface tension immersion being, in the two instances, in direct proportion to the respective values of the surface tensions. The mark 1·000 on the stem, therefore, floats a little distance above the surface of the mixture, and the hydrometer hence shows a reading which is higher than the actual specific gravity.

The length of the divisions and the diameter of the stem of any hydrometer being known, the error due to surface tension may be calculated.

The value of surface tension may be readily expressed in milligrammes for each millimetre of the circumference of the stem on which its acts. The surface tension of water is estimated by Van der Mensbrugghe as 7·3 milligrammes per mm. Other observers have found higher values, but it is difficult to obtain water sufficiently clean to exhibit even the surface tension of 7·3 milligrammes, for an exceedingly slight contamination of the surface by greasy matter suffices to appreciably reduce the tension.

The surface tensions of chloroform and benzol are very nearly equal

value, and may be taken in each case, for purposes of calculation, as
75 milligrammes per mm., the actual figures given by some other
servers varying slightly.*

I further find, by experiment, that the surface tension of a mixture
the two fluids of any specific gravity between 1·000 and 1·080 is,
r practical purposes, the same as that of the individual fluids.

The numerical difference between the values of the surface tensions
water and a chloroform and benzol mixture is therefore 4·55 milli-
ammes (7·3—2·75), and this, when multiplied by the circumference
xpressed in mm.) of the hydrometer stem, is equal to the weight in
illigrammes of a column of the mixture of the same diameter as the
em in question, and of a length which equals the extent to which
e stem is exposed below the specific-gravity mark which should be
e proper reading of the hydrometer.

This length may be calculated according to the simplified formula
$= 2\mathrm{T}/rw$, where h is height, T is surface tension, r is the radius of the
em, and w is the specific gravity of the fluid.

Having calculated this height in the case of a hydrometer immersed
a chloroform and benzol mixture of sp. gr. 1·000, the division of
is by the average length of the first divisions of the scale gives the
eoretical error (at this specific gravity) of the hydrometer, expressed
scale units.

ment that each instrument possessed an intrinsic error which tended to minimise the error of reading.

A hydrometer and its containing vessel were carefully cleansed with benzol and protected from contamination with greasy matter, and a sample of water taken, which was the cleanest readily obtainable, *i.e.*, tap-water which had been allowed to run through the pipe for some fifteen minutes. On immersing the hydrometer in this water, the mark 1·000 rested a slight distance below the surface. The water in which the hydrometer was standardised must have been contaminated, and hence possessed a considerably lower surface tension than that of the comparatively clean water in which my experiment was performed.

All the four hydrometers of my tables I found to possess this intrinsic error, which I estimated somewhat roughly, and have expressed in scale units. When these innate errors are added to the errors of Table I, the totals more closely approximate to the calculated errors. (See Table III.)

Table III.

Hydrometer.	Error due to difference between surface tensions of impure water and a chloroform-benzol mixture.	Error due to difference of surface tensions of impure and clean water.	Total error.	Calculated error.
1	0·002	0·0014	0·0034	0·0035
2	0·003	0·002	0·005	0·0056
3	0·0095	0·002	0·0115	0·0123
4	0·010	0·003	0·013	0·0146

There is thus sufficient agreement between the values of the observed and calculated errors to demonstrate that the disturbing influence of surface-tension is sufficient to cause the whole of the error in the hydrometer reading and to account for the inaccuracy of Hammerschlag's method. Taking into consideration the varying value of the surface tension of water, and the fact that no accurate determinations were made by me of each individual specimen of water or of chloroform and benzol, very exact calculations are precluded. Had this been done doubtless a more exact agreement between observations and calculations would have resulted. Furthermore, observations of this nature are replete with difficulties which can not be touched on here, but which may be gathered from a paper upon an elaborate investigation into a similar subject by Fridtjof Nansen.*

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* "Scientific Results of the Norwegian North Polar Expedition," vol. 3, Part 10

The difference in the error of the several hydrometers is readily ccounted for. A consideration of the facts already set forth shows hat the error when expressed in scale units must vary directly as the adius of the stem, and inversely as the total weight of the instrument, o that when the stem is fine in comparison with the weight, the error s small. But in making a small hydrometer it is impossible to keep lown the relative proportion of the stem. It thus follows that the maller hydrometers exhibit a greater surface tension error than the arger ones.

The greatest discrepancy which I have *observed* in any hydrometer in hloroform and benzol was 0·014. This instrument was a small one, raduated from 1·020 to 1·080, and is not included in the above ables.

This source of error in Hammerschlag's method may be obviated y :—

(1.) The estimation of the specific gravity of the chloroform and benzol nixture by means of an instrument which excludes or minimises the urface tension factors. The most convenient is some such balance as Vestphal's, in which the surface of the fluid is intersected by an xceedingly fine platinum wire only. The employment of hydrostatic ubbles is inconvenient on account of the long series required. The ienometer method is not readily applicable in connection with very

"Quaternions and Projective Geometry." By Charles J. Joly, F.T.C.D., Royal Astronomer of Ireland. Communicated by Sir Robert S. Ball, F.R.S. Received November 27,—Read December 11, 1902.

(Abstract.)

The object of this paper is to include projective geometry within the scope of quaternions. The calculus, as established by Hamilton, was solely adapted to the treatment of metrical relations, but when we regard a quaternion as representing a weighted point, projective properties can be investigated with great facility. Writing

$$q = \left(1 + \frac{Vq}{Sq}\right) Sq,$$

the point represented by the quaternion, q, is the extremity of the vector, Vq/Sq, drawn from an arbitrary origin, and the weight attributed to the point is Sq;* and this interpretation requires no modification in the principles of the calculus.

In this paper the theory of the linear quaternion function is developed to a considerable extent, and this theory is of fundamental importance, because the most general homographic transformation in space is expressible by means of a linear quaternion function. One section of the paper is devoted to the consideration of the scalar invariants of linear quaternion functions, and among these are included the invariants of systems of quadrics which correspond to the particular case in which the functions are self-conjugate. Moreover, in this section and more fully in the section on *covariance*, it is pointed out that the invariance in the case of the general functions is wider than in the case of self-conjugate functions. In fact, in the special case, the functions must remain self-conjugate after transformation. It is shown that there are in all eight distinct types of covariance.

The decomposition of linear transformations is also considered, and much use is made of the square-root of a linear quaternion function. A section is occupied with the determination of the linear transformations which shall convert given figures into other given figures, and with the conditions which in certain cases must be obeyed in order that such a transformation may be possible.

The general surface, the principle of reciprocity, generalised curvature and geodesics, are dealt with in a subsequent section, and the chief properties of an operator analogous to Hamilton's ∇ are exhibited in the section immediately following.

Several sections are occupied with the theory of the bilinear quater-

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* 'Trans. R. Irish Acad.,' vol. 32, pp. 1—16.

ion function, and this function is employed in the investigation of he properties of a four-system of linear transformations, of the eneral quadratic transformation, and of the non-linear one-to-one orrespondence of points in space. The method of quaternion rrays* is applied to the discussion of n-systems of linear transformaions, and of the critical assemblages of points, lines and planes conected with each system of transformations. Finally, in the conluding section it is explained how the method of the paper may be pplied to hyper-space, or to the discussion of functions of any umber of variables; and in many cases the formulæ obtained in the ourse of the paper with special reference to three dimensions require o modification to fit them for the general case of n variables.

"The Stability of the Pear-shaped Figure of Equilibrium of a Rotating Mass of Liquid."

By G. H. DARWIN, F.R.S Plumian Professor and Fellow of Trinity College, in the University of Cambridge.

Received and Read June 19 1902.

treated as a statical problem, if the mass be subjected to a rotation potential. The energy lost in the concentration of such a system from a condition of infinite dispersion consists of two parts. The first of these, say W, is the lost energy of the system at rest; the second is equal to the kinetic energy, say T, of the system in motion. The whole lost energy, say E, is equal to $W+T$, and the condition for a figure of equilibrium is that E shall be stationary for all variations, subject to constant angular velocity.

It might appear at first sight that the condition for secular stability is that E shall be a maximum. But M. Poincaré has shown that this condition is insufficient, and that it is necessary for stability that the whole energy, say U, which is equal to $-W+T$, shall be a minimum for all variations, subject to the condition of constancy of angular momentum.

He has, however, adduced another consideration, which enables us to determine the stability from the variations of E, without a direct consideration of the function U. He has shown, in fact, that if for given angular momentum slightly less than that of the critical Jacobian ellipsoid, from which the pear-shaped figures bifurcate, there is only one possible figure, namely, the Jacobian; and if for slightly greater angular momentum there are two figures, namely, the Jacobian and the pear,* then exchange of stability between the two series must occur at the bifurcation. If, on the other hand, the smaller momentum corresponds with the two figures and the larger with only one, one of the two (namely, the Jacobian) must be stable, and the other (namely, the pear) unstable.

The question is then completely answered by the value of the momentum of the pear; if it is greater than that of the critical Jacobian, the pear is stable, and if less, unstable. It suffices then to determine the pear from the variations of E with constant angular velocity, and afterwards to evaluate the angular momentum.

In the first approximation the pear-shaped figure is represented by the third zonal harmonic inequality with reference to the longest axis of the critical Jacobian ellipsoid. In proceeding to the higher approximation I suppose that its amplitude is measured by a parameter e, which is to be regarded as a quantity of the first order. We must now also suppose the ellipsoid to be deformed by every other harmonic, but with amplitudes of order e^2. In the first approximation W was proportional to e^2, but it now becomes necessary to go as far as the order e^4. A change in the sign of e means that the figure is rotated in azimuth through 180°. As this rotation cannot affect the energy, the odd powers of e must be absent from the expression for W. We have further to find the moment of inertia, as far as the terms f order e^2, and thence to find the kinetic energy T. The function E then equal to W + T.

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* For the sake of simplicity, I speak of one pear instead of two in azimuths differing by 180°.

In order to attain the requisite degree of accuracy it is convenient o regard the pear as being built up in an artificial manner.

I construct an ellipsoid similar to and concentric with the critical acobian, and therefore itself possessing the same character. The size f the new ellipsoid, which I call J, is undefined; and is subject only o the condition that it shall be large enough to enclose the whole ear. The region between J and the pear being called R, I suppose he pear to consist of positive density throughout J and negative ensity throughout R.

The lost energy of the pear consists of that of J with itself, say $\frac{1}{2}$JJ; f J with R, which is filled with negative density, say −JR; and of -R with itself, say $\frac{1}{2}$RR. This last contribution (which had baffled he) must be broken into several parts.

If we imagine J to be intersected by a family of orthogonal curves, nd if we suppose for the moment that the region R is filled with ositive matter, we may further imagine the matter lying inside any rthogonal tube to be transported along the tube, and deposited on the urface of J in the form of a concentration of positive surface density -C.

the pear above the kinetic energy which it would have had if it rotated with the angular velocity of the critical Jacobian. If ω denotes the latter angular velocity, and $(\omega^2 + \delta\omega^2)^{\frac{1}{2}}$ the actual angular velocity of the pear; if A_j, A_r denote the moments of inertia of J, and of R considered as filled with positive density, we have

$$E = \tfrac{1}{2}JJ - JR + CR - \tfrac{1}{2}CC + \tfrac{1}{2}DD + \tfrac{1}{2}(A_j - A_r)\,\delta\omega^2.^*$$

The co-ordinates of points are determined by reference to the ellipsoid J which envelopes the whole pear. The size of J is indeterminate, and therefore the formulæ must involve an arbitrary constant expressive of the size of J. But the final result for E cannot in any way depend on the size of the ellipsoid which is chosen as the basis for measurement, and therefore the arbitrary constant must ultimately disappear. Hence it is justifiable to treat it as zero from the beginning, and we may use the formula for the internal gravity throughout the investigation.†

Although the constant expressive of the size of J is put equal to zero—which means that the pear is really partly protuberant beyond the ellipsoid—yet there is a considerable amount of mental convenience in continuing to discuss the subject as though the ellipsoid completely enveloped the pear.

When an ellipsoid is deformed by an harmonic inequality, the volume of the deformed body is only equal to that of the ellipsoid, to the first order of small quantities. In the case of the pear, all the inequalities, excepting the third zonal one, are of the second order, and as far as concerns them the volumes of J and of the pear are the same. But it is otherwise as regards the third zonal harmonic term, and the first task is to find the volume of such an inequality as far as e^2. When this is done, we can express the volume of J in terms of that of the pear, which is of course a constant.

By aid of ellipsoidal harmonic analysis we may now express the first four terms of E in terms of the mass of the pear and of certain definite integrals which depend on the shape of the critical Jacobian ellipsoid.

The energy $\tfrac{1}{2}DD$ presents much more difficulty, and it is especially in this that M. Poincaré's insight and skill have been shown. The system D consists of a layer of negative volume density coated on its outer surface with a layer of surface density of equal and opposite mass. His procedure virtually amounts to regarding this system as consisting of an infinite number of magnetic layers, whose energy may be evaluated and summed. The reduction of this part of the energy to calculable forms is not very simple.

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* A term depending on the shift of the centre of inertia proves to be negligible.

† Compare with M. Poincaré's treatment of the same point, 'Phil. Trans.,' A, vol. 198, p. 352.

On the ‘Blaze-currents’ of the Incubated Hen's Egg." By AUGUSTUS D. WALLER, M.D., F.R.S.

Received July 17,—Read December 4, 1902.

(From the Physiological Laboratory of the University of London, S.W.)

In previous communications to the Society, on the Eyeball,* on the kin,† and on Leguminous Seeds,‡ I have reported the results of experinents conducted by aid of an electrical criterion distinguishing between he living and not-living state.

The present communication contains the results of a series of systenatic observations on the hen's egg by aid of the same distinguishing est—or blaze reaction, as I was led to designate it when it first came nder my observation in the case of the frog's eyeball.

The case of the hen's egg is particularly interesting, for while we annot tell *a priori* with any assurance whether or no a dormant egg vill give the reaction characteristic of living matter, we may—after aving learned by experience that it does *not* do so—expect to find he reaction make its appearance with the progress of development by ncubation. And as a matter of fact, we find that this is what appens.

Egg No. 3, also at the end of 48 hours, gave large effects in both directions, larger and more persistent in the upward than in the downward direction. The difference between the reactions of these two eggs was in obvious correlation with their unequal degree of development, for whereas in No. 2 an area vasculosa was only just apparent at one border, in No. 3 it was well formed, and the heart was observed pulsating for more than 12 hours after exposure of the blastoderm.

No. 4 (72 hours) reacted well in both directions, and was normal.

No. 5 (72 hours) gave me pause. In spite of repeated trial I could not obtain a trace of the reaction that I expected to obtain. Every stimulus of whatever strength and direction gave rise to a slight counter-effect. But the explanation of the result was forthcoming when the egg was opened. No development whatever had taken place.

No. 6 (72 hours) gave normal reaction in both directions. Development was normal.

No. 7 (96 hours), normal reactions and normal development. Reaction abolished by rise of temperature with embryo exposed.

No. 8 (108 hours), normal reactions and normal development. Reaction abolished by rise of temperature.

No. 9 (144 hours), normal reactions and normal development. Reaction abolished by injection of a 2·7 per 100 solution of mercuric bichloride.

No. 10 (12th day of incubation) gave no blaze in either direction, only polarisation. The contents of the egg were rotten.

The above series of results was evidently in accordance with the fundamental fact. There was no exception to rule in any of the ten trials.

Certain of these ten observations were taken in closer detail in order to get at information as to relation between stimulus and response, effect of strong electrical stimulation, &c., and although these matters will demand considerable further investigation, some of the results may be described now.

This first series, by no means satisfactory from a chicken-farmer's point of view, was practically conclusive for my purpose, which was to learn whether the presence or absence of a living embryo could be diagnosed from the presence or absence of blaze-currents.

Other trials made at times of year still more unfavourable as regards probability of development, viz., in August and in November, gave results that were equally satisfactory from my point of view.

Thus in the second series, No. 1 after 32 hours' incubation gave small blaze effects of ± 0·0010 volt in both directions; the blastoderm was the size of a sixpence, and there was no sign of circulation.

nd 3 were used in the absence of incubation for a careful examination f the exposed blastoderms, one at normal temperature, the other at 8°; polarisation currents were alone observable in these two cases, mounting to ± 0·0002 volt. Nos. 4, 5, and 6 at 50 and 58 hours ave positive blaze of only 0·0030 and 0·0010 volt, and their blastoerms were found to be very defective. Nos. 6, 7, 8, 9, and 10 gave no laze whatever, only the usual small counter-deflections of ± 0·0002 olt due to polarisation, and on being opened exhibited no sign of evelopment.

Incidentally to this first series of trials I noted that:—

1. The "normal current," from a developing egg, led off as described, positive or ascending, and if the egg is left undisturbed diminishes uring observation. I consider it to be a "manipulation blaze" ue to handling of the egg and disturbance of the embryo. It is resumably the current first pointed out by Hermann and von Gendre their statement that the embryo is positive to any other part of he egg contents, *i.e.*, that there is a current from content to mbryo.*

FIG. 1.

shocks at intervals of 5 or 6 minutes. By strong tetanisation the reaction is completely abolished; the chick has been "electrocuted."

In a third series of ten trials upon "fresh eggs" from a London shop, and at a very unfavourable time of year as regards development, the results were as clear as could be desired. At the end of 12 days of incubation nine of these eggs returned what was by this time familiar to me as a negative answer, viz., small counter-currents due to polarisation, and all the nine showed no sign of development; only a single egg gave blaze-currents of ± 0·0010 volt, and was found to have developed to the extent usual at the end of 24 hours under ordinarily favourable conditions.

In the following year (1902) I returned to the subject and made two further series of observations, paying particular attention to the direction of blaze-currents of the first few days of incubation. In the interval between these observations and those of the previous year I had studied the currents of mucous membranes, with the general, but by no means invariable, result that the blaze-currents are of ingoing direction. I therefore expected to find, and did find, that the response of an early blastoderm to either direction of current is of positive or ascending direction, *i.e.*, ingoing as regards the hypoblast, and outgoing as regards the epiblast.

I also took the opportunity of testing active eggs on what has been described in previous papers* as the A B C plan, *i.e.*, after excitation through A, the superior pole, and B, the inferior pole of the egg, the lead-off to the galvanometer through A C was found to be effective (outgoing current at A), and the lead-off through B C ineffective. The response was outgoing or positive at A after both directions of excitation between A B.

The method of observation is further illustrated by the following table and plates. Plates 1939–40 taken on the 6th egg (72 hours) are given in detail to show how the magnitude of response varies with

1939–40. Chick Embryo. 72 hours.

Arithmetic Increase of Quantity and of Energy by increasing Capacity at Constant Voltage.

Capacity.	Pressure.	Quantity.	Energy.	Response.
1 mf.	8·4 volts.	8·4 mc.	360 ergs.	0·0010 volt.
2 "	8·4 "	16·8 "	720 "	0·0018 "
3 "	8·4 "	25·2 "	1080 "	0·0022 "
4 "	8·4 "	33·6 "	1440 "	0·0025 "
5 "	8·4 "	42·0 "	1800 "	0·0027 "

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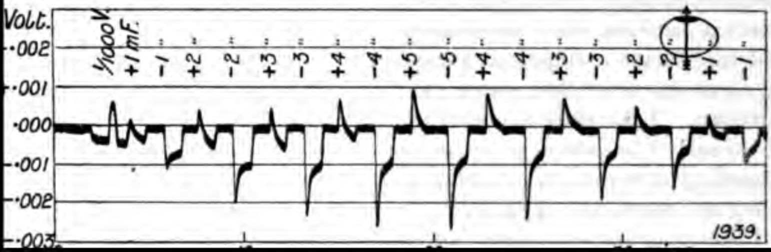
* Roy. Soc. Proc.,' vol. 68, p. 488; vol. 69, p. 181.

e magnitude of its exciting cause. The response appears to depend energy rather than on quantity of electrical stimulus; plate 1948 ken on the 8th egg (108 hours) shows this point even more clearly. he same chick was used for examination of the influence of mperature.

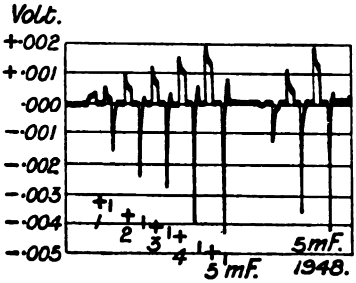
rithmetic Increase of Quantity and Geometric Increase of Energy by increasing Voltage at Constant Capacity.

5 mf.	2·8 volts.	14 mc.	200 ergs.	0·0005 volt.
5 ,,	5·6 ,,	28 ,,	800 ,,	0·0011 ,,
5 ,,	8·4 ,,	42 ,,	1800 ,,	0·0013 ,,



Chick, 108 hours.

Pressure.	Capacity.	Quantity.	Energy.	Response.
6 L.	1 mf. +	8·4 mc.	360 ergs.	+0·0005
"	" −	..	..	−0·0016
"	2 mf. +	16·8	720	+0·0009
"	" −	..	..	−0·0025
"	3 mf. +	25·2	1080	+0·0011
"	" −	..	..	−0·0032
"	4 mf. +	33·6	1440	+0·0015
"	" −	..	..	−0·0040
"	5 mf. +	42·0	1800	+0·0019
"	" −	..	..	−0·0043
2 L.	5 mf. +	14·0	200	+0·0000
"	" −	..	..	−0·0015
4 L.	" +	28·0	800	+0·0010
"	" −	..	..	−0·0036
6 L.	" +	42·0	1800	+0·0019
"	" −	..	..	−0·0042



July 25. Chick, 108 hours.

Influence of Temperature.

Exc. by Condenser discharge. 5·6 volts; 4 L 5 mf. +

Time.	Temp.	Blaze.	Resistance.
0	28°	+0017	50,000 ω
5	28	0016	—
10	34	0014	—
15	38·5	0009	—
20	41·5	0004	35,000
25	44°	0000	—
35	37	0000	—
45	31	0000	35,000

Summary.

The presence of a blaze-current is a certain sign that development has progressed within the egg.

In the early stages—when presumably the blastodermic membrane as not yet become folded to form a tubular embryo—the blaze-urrents aroused by both directions of excitation are positive or scending.

At a more advanced stage of development the blaze-currents are sually homodrome with the direction of excitation, viz., positive or scending after a positive or ascending excitation, and negative or escending after a negative or descending excitation.

In some cases, but with such infrequency as to deserve to be characerised as exceptional, both responses have been observed to occur in negative or descending direction. This may have been due to ttachment of the embryo to the shell.

[I have made a few observations on frog's spawn, and although these ave not yet been sufficient to enable me to specify the conditions of he presence or absence of the reaction, I think that the fact of its resence is worth reporting, as well as the further fact that in some ases of undoubtedly living spawn I have failed to detect it.]

Fourth Series. June 1902. Berne Coil. 2 Leclanché's. 10,000 units.

			Single break induction shocks.		Tetanising currents.		State found on opening the egg.
			br. +	br. −	{m − br +	{m + br −	
1st egg	Shell broken	46 hours ..	+0·0020	+0·0010	+0·0055	+0·0030	Normal.
2nd „	„ unbroken	46 „ ..	−0·0030	+0·0010	+0·0030	+0·0025	Normal.
	„ broken	46 „ ..	+0·0025	+0·0014	+0·0040	+0·0015	Normal.
3rd „	„ „	46 „ ..	+0·0003	+0·0002	+0·0006	+0·0005	Normal. (Strong tetanisation before test was made.)
4th „	„ unbroken	46 „ ..	−0·0007	−0·0002	—	—	Doubtful result.
	„ broken	..	+0·0012	+0·0005	+0·0040	+0·0022	Normal.
5th „	„ „	66 „ ..	+0·0025	+0·0015	+0·0090	+0·0045	Normal. Cirra vasculosa well-developed.
6th „	„ „	90 „ ..	+0·0040	+0·0010	+0·0120	+0·0035	Normal.
7th „	„ unbroken	4th day ..	+0·0022	+0·0010	+0·0100	+0·0006	Strong tetanisation (8 Lecl. 10,000).
	Same egg	8th „ ..	trace −	trace +	+0·0005	−0·0005	Embryo dead and smelling—killed presumably by strong tetanisation.
8th „	Shell broken	5th „ ..	+0·0030	+0·0007	+0·0125	+0·0050	Normal.
	Egg boiled	..	−0·0002	+0·0024			
9th „	Shell unbroken	6th „ ..	+0·0003	+0·0001	+0·0010	+0·0000	Doubtful result.
	„ broken	..	? trace	? trace	? trace	? trace	No sign of development. Bad smell.
10th „	„ „	7th „ ..	+0·0008	−0·0002	+0·0030	+0·0020	Normal.
11th „	„ „	9th „ ..	nil	nil	nil	nil	Embryo dead and smelling.
12th „		10th „ ..	−0·0010	−0·0015	−0·0030	−0·0060	Well developed chick. All responses in descending direction. All responses of both eyes were positive, +0·0010 to +0·0015.
13th „	„ „	3rd „ ..	nil	nil	trace +	trace −	No development.
14th „	„ „	3rd „ ..	+0·0060	+0·0018	+0·0060	+0·0020	Normal.
15th „	„ „	3rd „ ..	+0·0006	+0·0002	+0·0016	+0·0002	Normal.
16th „	„ „	5th „ ..	nil	nil	nil	nil	No development. Bad smell. N.B. These eggs had not been turned or kept moist in the incubator for 3 or 4 days.
17th „	„ „	5th „ ..	nil	nil	nil	nil	
18th „	„ „	5th „ ..	nil	nil	nil	nil	
19th „	„ „	5th „ ..	nil	nil	nil	nil	
20th „	„ „	5th „ ..	nil	nil	nil	nil	

The numbers in these four columns signify voltage of response.

esponse.	
nil ..	Only polarisation counter-currents in both directions. Same with exposed yolk.
nil ..	
mall + ..	Small ascending blaze.
nil ..	
mall + ..	Scarcely developed vascular area. Small ascending blaze.
nil ..	
010 volt ..	Well-developed vascular area. Pulsating cardiac speck. Ascending blaze.
006 ,, ..	
022 ,, ..	Pulsation still visible. 12 hours later. Ascending blaze.
011 ,, ..	
005 ,, ..	4 hours later. Pulsation just visible.
010 ,, ..	
ge	Response in both directions. Well-developed embryo.
ge	
all	No development. No blaze. Only polarisation.
all	
all	
all	
010 volt ..	Good vascular area and embryo. (*In extenso.*) Ascending and descending blaze.
026 ,, ..	
019 ,, ..	
023 ,, ..	
028 ,, ..	Good embryo. Ascending and descending blaze.
044 ,, ..	Temperature raised to 40°. Reactions abolished.
015 ,, ..	
045 ,, ..	
nil ..	
nil ..	
010	Ascending and descending blaze. (*In extenso.*) Temperature raised to 44°. Reaction abolished.
036	
nil ..	
nil ..	

1949	,, 27	9th egg (144 hours)	6 L., 5 mf. +	+0·0025	Ascending and descending blaze before $HgCl_2$.
,,	,, ,,	,,	6 L., 5 mf. −	−0·0050	
Unrec.	,, ,,	,,	6 L., 5 mf. +	nil	After injection of $HgCl_2$ sol. (N/10) abolished.
,,	,, ,,	,,	6 L., 5 mf. −	nil	
,,	,, ,,	,,			
,,	Aug. 2..	10th egg. 12th day	6 L., 5 mf. +	− small	Bad egg. No blaze. Only polarisation.
			6 L., 5 mf. −	+ small	
			Br O +	− small	
			Br O −	+ small	

Second Series (1901).

,,	,, 3..	1st egg (32 hours incubated)			
			6 L., 1 mf. +	+ small	Blastoderm size of sixpence. No vascular area.
			6 L., 1 mf. −	− small	
1976	—	,, ,, ,,	6 L., 1 mf. +	+0·0012	Ascending and descending blaze before tetanisation.
			6 L., 1 mf. −	+0·0008	
1977	—	,, ,, ,,	6 L., 1 mf. +	+0·0040	
			6 L., 1 mf. −	−0·0007	No further response after strong tetanisation.
Unrec.	—	2nd egg, fertile but not incubated	6 L., 5 mf. +	−0·0002 (about)	Blastoderm exposed. Only polarisation. No blaze.
			6 L., 5 mf. −	+0·0002 ,,	
,,	—	3rd egg, do. do., and warmed to 37°	6 L., 5 mf. +	−0·0002 ,,	Blastoderm exposed. Only polarisation. No blaze.
			6 L., 5 mf. −	+0·0002 ,,	
	—	4th egg (49½ hours)	Br O +	+0·0030 ,,	Only ascending blaze. Area vasculosa distinct.
			Br O −	+0·0010 ,,	
,,	,, 5..	5th egg (58 hours)	Br O +	+0·0002 ,,	Only ascending blaze.
			Br O −	+0·0001 ,,	
			Br O +	+0·0010 ,,	Normal current ascending + 0·0080 volt. Only ascending blaze. Area vasculosa distinct.
			Br O −	+0·0002 ,,	
,,	,, ,,	6th egg (59 hours)	6 L., 5 mf. ±	± nil	
			Br O +	+0·0012	Only ascending blaze.
			Br O −	+0·0001	
			Br O +	+0·0015	Only ascending blaze. Normal current was + 0·0035 (ascending).
			Br O −	+0·0002	
	,, 9..	7th egg (125 hours)	6 L., 5 mf. ±	nil (polarisn. ±)	No development whatever.
			Br O ±	nil (polarisn. ±)	No development whatever. R = 26,500 ω.
	,, ,,	8th egg (90 hours)	Br O ±	nil	
		9th egg (90 hours)	Br O ±	nil (polarisn. ±)	No development whatever.
		10th egg (90 hours)	Br O ±	nil (polarisn. ±)	R through shell = 1,000,000, falling to 400,000 ω.

On the 'Blaze-currents' of the Crystalline Lens." By A. D. Waller, M.D., F.R.S., assisted by A. M. Waller. Received October 23,—Read December 4, 1902.

(From the Physiological Laboratory of the University of London, S.W.)

In the course of investigation of the effects of light and of electrical xcitation on the frog's eyeball, I came to the conclusion that tissues ther than retinal are coeffective in the response to strong induction hocks, and proceeded therefore to look for blaze-currents in other ving tissues.*

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* 'Phil. Trans.,' B, 1901, vol. 194, p. 185.

The following extract from my note book of 1900 gives instances in which the action of the anterior half of the eyeball was observed to *exceed* that of the sterior half.

Frog's eyeball, entire and bisected.

Excitation by single break shock from Berne coil.

Strength of excitation.	Whole eyeball.	Posterior half.	Anterior half.
1000 +	+ 0·0013 volt.	Nil	+ 0·0030 volt.
1000 −	+ 0·0026 „	„	+ 0·0033 „

nother eyeball—

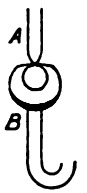
times found to give a larger response than the posterior half, and the present observations proceed from an attempt to determine the principally effective part in such reaction. And I may state at once, as my chief conclusion, that it is the crystalline lens.

The eyes upon which the determination was made, in the first instance, were those of fish—whiting and mackerel—by reason of the fact that these were for a season at my disposal quite fresh from the sea. I subsequently made similar observations on the eyes of octopus, on sheep's eyes fresh from the slaughter-house, and on the eyes of recently killed cats and rabbits; also on the eyes of an owl.

The point that was most striking in these first observations was the great endurance of the reaction in the crystalline lens as compared with its rapid disappearance from the remaining tissues of the eyeball and from the skin, and with the rapid disappearance of the direct electrical excitability of muscle. I should, as an outcome of these observations, look for the last sign of life of a fish by testing the crystalline lens, whereas in the case of man I should test a piece of skin. The reaction—as far as I have yet seen—has been completely absent from frozen fish (salmon) as received from London fishmongers. Its normal direction in the lens is "negative," *i.e.*, from external to internal pole. It is abolished by heat (70°) and by compression.

My first experiments with the eyes of fish were to ascertain on the entire eyeball what type of blaze reactions—if any—is manifested The results were as follows:—

Exp. 1. *Whiting.*—Excit. and lead-off through AB. Berne coil. Two Leclanchés in primary circuit. Single break induction shocks.

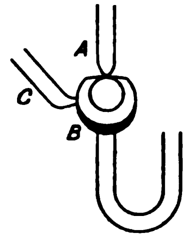


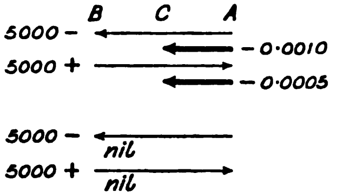
1000 + gave − 0·0004 volt.
1000 − „ − 0·0007 „
5000 + „ − 0·0010 „
5000 − „ − 0·0017 „

Reactions after immersion in hot water.

5000 + gave nil.
5000 − „ „

Exp. 2. *Whiting.*—Excit. through AB and lead off through BC or AC. (The system of notation is explained in the 'Roy. Soc. Proc.,' vol. 69, p. 183.)





Response from A to C; no response from B to C.

Lens alone.	Exc. 1000 +	gives	– 0·0025
	1000 –	„	– 0·0060
Cornea alone.	Exc. 5000 +	„	nil.
	5000 –	„	– 0·0005
2nd lens alone.	Exc. 1000 +	„	– 0·0020
	1000 –	„	– 0·0050

Completely abolished after immersion in hot water.

A similar experiment gave similar results: in the first lens the esponse was completely abolished by compression, in the second lens t was greatly diminished and modified by tetanisation; the response o – + excitation being at the outset – –, then – +, then + +.

Excitation	←——	——→
Response I	←——	←——
„ II	←——	——→
„ III	——→	——

Exp. 3. *Mackerel.*—About 5 hours after death. Lens alone.

1st lens. Exc. 1000 + gives nil.

After compression no further response.

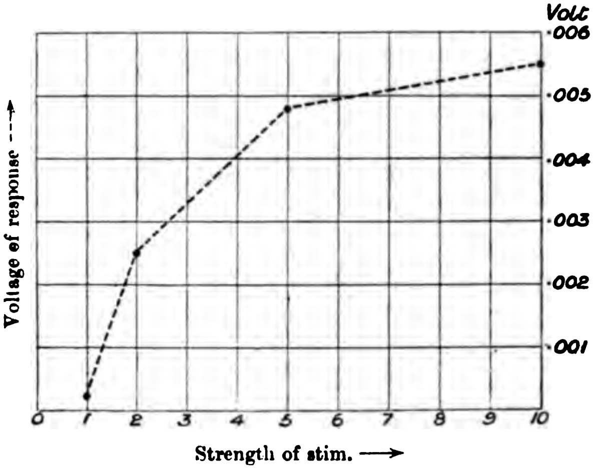
There was no appreciable alteration of resistance after tetanisation.

Exp. 5. *Whiting.*—4 hours *post mortem.* Isolated lens.

1000 +	gives	+ 0·0002
1000 −	„	− 0·0010

		↓	↑
500 +	gives	nil	nil
1000 +	„	+ 0·0002	+ 0·0002
2000 +	„	+ 0·0025	+ 0·0020
5000 +	„	+ 0·0050	+ 0·0040
10000 +	„	+ 0·0055	+ 0·0050

No further response after compression.



Exp. 6. *Mackerel.*—48 hours *post mortem.* Isolated lens.

Exc.	1000 +	gives	− 0·0003 volt.
	1000 −	„	− 0·0004 „
	10000 +	„	− 0·0010 „
	10000 −	„	− 0·0015 „

The response is abolished by plunging lens in hot water.

Exp. 7. *Octopus.*—15 hours after removal from water.

The *isolated eye* gives no distinct response either to light or to electrical excitation.

Its isolated lens gives to

1000 +	a response of	+ 0·0040 volt.
1000 −	„	+ 0·0005 „
5000 +	„	+ 0·0015 „
5000 −	„	+ 0·0010 „

Later and with altered position of lens on electrodes.

1000 +	a response of	– trace.
1000 –	,,	– 0·0004 volt.
5000 +	,,	– 0·0005 ,,
5000 –	,,	– 0·0010 ,,

Exp. 8. *Dogfish.*—Isolated lens.

1000 +	gives	– 0·0012 volt.
1000 –	,,	– 0·0020 ,,

The response is abolished by pressure.

Exp. 9. *Cuckoo Fish* (white).—Some hours (? 4 or 5) after death. The eyeball gives no response to light or to electrical excitation.

Its lens, to 5000 +	gives	– 0·0020 volt.
5000	,,	– 0·0050 ,,

Set up in connection with three electrodes so as to be excited through AB, and led off through AC or BC (as for the entire eyeball). The lens responses are as follows:—

B C A

Exp. 12. *Codfish* (1st).—Isolated lens. Fish said to have been brought to shore on the previous day.

5000 –	gives	– 0·0040,	– 0·0020
5000 +	"	– 0·0010,	– 0·0010

Photo. 4242 is now taken—

5000 +	"	– 0·0009
5000 –	"	– 0·0022

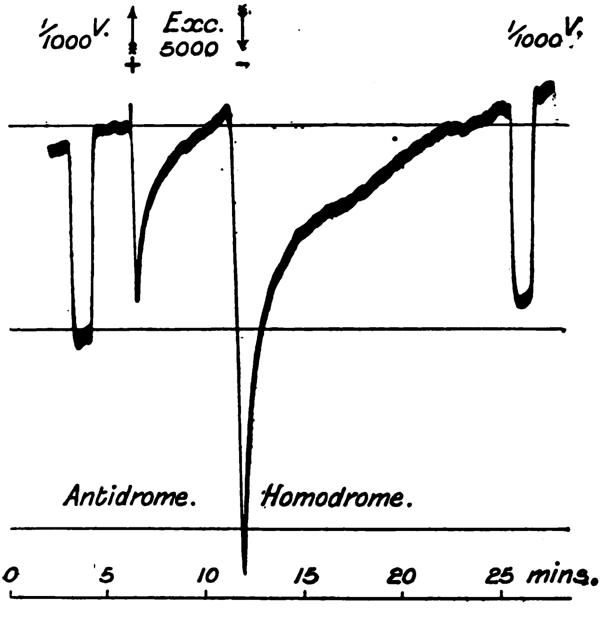


Photo. 4242.—Codfish Lens. Anti- and homodrome responses.

The normal current was – 0·0008.

The other lens gave no distinct response.

The same lens next day gave

to 5000 +	– 0·0005
–	– 0·0015

abolished by heat.

Exp. 13. *Codfish* (2nd).—Lens. 1 or (?) 2 days after capture of the fish, tested by the ABC method for determination of the seat of the response. (Given *in extenso.*)

Exp. 14. *Codfish* (3rd).—Lens. ? second day after capture.

5000 +	– 0·0007
5000 –	– 0·0033

10000 +	– 0·0013
10000 –	– 0·0048
10000 –	– 0·0052
10000 +	– 0·0015
5000 –	– 0·0027
5000 +	– 0·0008

Moderate compression gives – deflection off scale in consequence of echanical excitation. Considerable compression abolishes all re-ponse.

The other lens gave no response; the eye from which it had been emoved was evidently injured, being full of blood.

Exp. 15. *Codfish* (4th).—Supplied as fresh; neither lens gave any esponse.

Exp. 16. *Mackerel.*—Lens. Fish reputed fresh.

5000 +	– 0·0004
5000 –	– 0·0011
Moderate compression	– ——
5000 +	+ 0·0002
5000 –	+ 0·00015
Severe compression	– ——

Then photo. 4248—

5000 +	− 0·0010
„ −	− 0·0014
„ +	− 0·0005
„ −	− 0·0008

Exp. 19. *Cat.*—Lens. 1½ hour *post mortem.*

1st lens	5000 +	− 0·0003
	„ −	− 0·0008

Response abolished by compression.

2nd lens	5000 +	− 0·0002
	„ −	− 0·0006
Same lens next morning	+	− trace
	−	+ trace

Exp. 20. *Mackerel.*—Lens. Reputed fresh.

1000 +	+ 0·0002
−	− 0·0001
5000 +	+ 0·0003
−	− 0·0004
10000 +	+ 0·0004
−	− 0·0006

Responses homodrome throughout.

Exp. 21. 5*th Codfish.*—Lens.

1st lens	5000 +	− 0·0002	
	−	+ 0·0001,	− 0·0002
	10000 −	− 0·0006	
	+	− 0·0002,	+ 0·0003
2nd lens	10000 +	− 0·0001	
	−	+ 0·0001	

Attempts were made to test the last two lenses by lateral eye rotation; the results were uncertain and variable. Similar trials in other cases were equally variable.

Exp. 22. *Cat.*—Lens. 5 hours *post mortem.*

1st lens.	Initial current	+ 0·0011	
	1000 + and −	gave	nil, nil.
	5000 + „ −	„	− 0·0004 and nil.
2nd lens	5000 +	„	− 0·0015
	5000 −	„	− > 0·0030 (off scale).

Exp. 23. *Brill.*—Lens (? 24 hours).

Initial current		− 0·0036
Exc. by single break	1000 +	− 0·0020

Q 2

	1000 –	– 0·0080
	100 +	– 0·0003
	100 –	– 0·0015
After compression	1000 + and –	nil, nil.
Electrodes tested by	1000 + ,, –	,, ,,

Note.—The lens of this fish is rather smaller than convenient.

Exp. 24. *Frog.*—R. temporaria.

Entire eyeball. Initial current		=	– 0·0030
Exc. by single break	100 +	=	+ off (>0·002)
,, ,,	100 –	=	+ off (>0·002)
Its isolated lens.	100 + and –	=	nil, nil.
	1000 +	=	– 0·0005
	1000 –	=	– 0·0010

Subsequently both responses were observed to be homodrome, viz., + to + exc., and – to – exc.

The isolated lens of the other eyeball gave similar results.

The lens of the frog's eye is inconveniently small, nevertheless, with ue care, typical effects can be observed upon it, viz., negative esponses* to both directions of excitation, the homodrome exceeding unequally good; in three instances one of the lenses gave no response, and in one of these three instances the eyeball was filled with blood. I think the difference between the two eyes must have been due to the fish having been killed by stunning, or it may be that in transit to London one of the eyes had suffered compression. But whatever the real cause of the difference may have been, the lenses of fish obtained in London were far less satisfactory than those of fish directly taken from the sea. In the latter case, both lenses, if carefully removed, were equally effective (provided the fish had not been stunned in the usual way on removal from the hook).

Similar effects are obtainable on the crystalline lens of the mammalian eye; but it is essential to avoid any undue compression of the globe. Thus I completely failed to observe any effect on the lens of eyes removed from the orbit of dogs and cats in the usual manner, also on the lens of sheep's eyeballs brought fresh from the slaughterhouse.

But with lenses carefully removed from the eyes of a fresh sheep's head and of a recently killed cat, typical and regular responses were obtained, which were abolished by intentional compression as well as by immersion in hot water (60° to 70°).

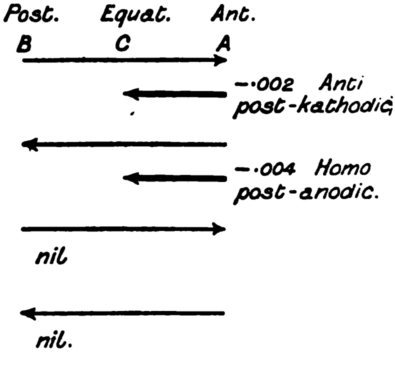
I think it desirable to give *in extenso* one experiment (No. 13) to illustrate the precise nature of experimental evidence and the system on which it is taken down. It is very easy to make sure of the direction of a current used for excitation in relation to a total or bipolar response, but it is not easy without a strictly systematic plan to make sure of this relation when a partial or unipolar response is under investigation. It is advisable for the latter purpose to carefully verify the connections of the ABC key* so that directions of deflection may immediately signify directions of current between the points of investigation, and be noted accordingly in a legible form that can be readily reviewed.

Exc. 5000 + through BA
Response from AC

Exc. 5000 − through BA
Response from AC

Exc. 5000 + through BA
Response from BC

Exc. 5000 + through BA
Response from BC



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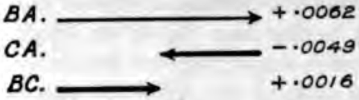
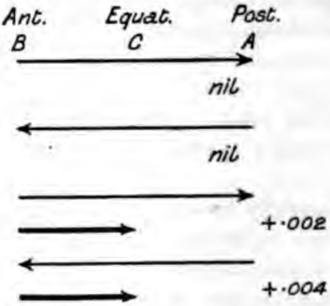
* Described in 'Roy. Soc. Proc.,' vol. 69, p. 181.

I give the normal currents, not that they are essential, but because an expert reader might wish to know them.

Normal, *i.e.*, accidental current



All response was from the anterior pole; none from the posterior. The series was repeated with similar results, and now the zincs of the electrodes were transposed so that the connections were—



i.e., as before response only from anterior and not from posterior pole.

The lens is submitted to compression, after which there is no response of any kind, either total or partial; it is placed for a few minutes in hot water until coagulated white, and again tested without any response. The temperature at which the first obvious sign of coagulation was observed was 38°. The lens was completely white at 48°. Beyond 50° no further increased whiteness could be seen.

I conclude from this and similar experiments—

1. That a crystalline lens of suitable size is a good object upon which to study the nature of blaze-currents.
2. That a "blaze-current" is a physical sign of the "living" state.
3. That a blaze-current may be post-kathodic as well as post-anodic, antidrome as well as homodrome.
4. That the direction of blaze-currents in the lens is negative or ingoing, *i.e.*, from external or anterior to internal or posterior pole.

ADDED DECEMBER 4, AFTER THE RECEIPT OF DR. DURIG'S PAPER. (See p. 212.)

Exp. 25. Nov. 1. *Owl.*—Positive responses of the anterior half of the eyeball; negative responses of the lens alone; positive responses of the cornea alone.

1st eyeball. *Anterior half.* 7 minutes *post mortem.*

Initial current = −0·0008 to −0·0018 volt.

Exc. by single break	1000 +	+0·0005
" "	−	+0·0005
" "	5000 +	+0·0020
" "	−	+0·0015
" "	−	+0·0015
" "	+	+0·0015

Lens alone.

10000 +	−0·0005
−	−0·0020

Cornea alone.

Single break	10000 +	nil.
"	10000 −	nil.
Several	10000 +	+0·0012, +0·0010
"	10000 −	+0·0020, +0·0020
Tetanisation	10000 +	+0·0015
"	10000 −	+0·0025

Cornea plunged in hot water.

Tetanisation	10000 −	nil.
"	10000 +	+0·0005

2nd eyeball. *Anterior half.* 2 hours 10 minutes *post mortem.*

Initial current		= +0·0005 to nil.
Exc. by single break	5000 +	+0·0010 volt.
" "	−	+0·0015 "

Lens alone.

Exc. by single break	5000 +	− >0·0030 (off scale)
" "	−	− >0·0030 "
" "	1000 +	− 0·0004
" "	−	− 0·0010

Cornea alone.

Single break	5000 +	nil.
"	−	+0·0007
"	10000 +	nil.
"	−	+0·0009
Several	10000 +	nil.
"	−	+0·0010

Lens replaced.

Single break	1000 +	−0·0007
"	−	−0·0012

	700 +	− 0·0013	antidrome blaze
	−	> − 0·0028	homodrome "
	600 +	− 0·0008	antidrome "
	" −	> − 0·0028	homodrome "
	" +	− 0·0006	antidrome "
	" −	− 0·0020	homodrome "
	+	− 0·0004	antidrome "
	−	− 0·0018	homodrome "
	+	− 0·0003	antidrome "
	−	− 0·0011	homodrome "
½ hour interval.			
	600 −	− 0·0031	homodrome "
3rd day. 2nd lens (Nov. 10)			
	600 −	> − 0·0038	homodrome "
	500 +	− 0·0011	antidrome "
	" −	− 0·0038	homodrome "
	400 +	− 0·0003	antidrome "
	" −	− 0·0016	homodrome "
4th day. 2nd lens (Nov. 11)			
	400 +	− 0·0008	antidrome "
	−	− 0·0006	homodrome "
	500 +	− 0·0005	antidrome "
	−	− 0·0005	homodrome "
	400 +	− 0·0001	antidrome "
	−	− 0·0002	homodrome "
5th day. 2nd lens (Nov. 12)			
	500 +	nil.	
	−	nil.	
	1000 +	nil.	
	" −	nil.	
	5000 +	− 0·0001	polarisation
	" −	+ 0·0002	
Tetanise	10000 +	+ 0·0010	blaze ?
	" −	− 0·0010	
6th day. 2nd lens (Nov. 13)			
Tetanise, 12 P.M.	10000 +	− 0·0005	blaze ?
	" −	− 0·0006	
7th day. " 2 P.M.	10000 +	+ 0·0007	polarisation
	−	− 0·0008	
Sing. shock	10000 +	− 0·0001	polarisation
	−	+ 0·0002	

This is the longest period during which I have followed the response of an isolated lens.

Indubitable response, negative to positive (antidrome) and negative o negative (homodrome), to single shocks, was observed on the 4th lay, absent on the 5th day.

Response, negative to negative, with strong tetanisation in both airs of directions, was still present on the 5th and 6th days. But on he seventh day the only visible effects were of polarisation direction, .*e.*, the lens was judged to be dead.

Exp. 27. (November 19, with Sir J. Burdon-Sanderson.)—Lens of abbit eye, removed 4 hours *post mortem*. Normal current 0·0150 volt ositive declining.

1*st lens.*

Excitation by single shock,

Coil at 1000 + = − 0·0006
− = − 0·0010 followed by positive after-effect.

Excitation with the ABC method.
By single induction shocks.

(1st day).

B C A

Coil at 5000

Exc. ⟶
⟵ = −0·0008
Exc. ⟵
⟵ = −0·0013

S.s. 5000 Exc. ⟶ +
Resp. ⟵ = −0·0004

Exc. —— −
Resp. ⟵ = −0·0008

Next day. 1st lens.
(2nd day.)
S.s. 5000 Exc. ⟶ +
Resp. ⟵ = −0·0021

Exc. ⟵ −
Resp. ⟵ = −0·0006

Exc. —— +
Resp. ⟵ = −0·0016

Exc. ⟵ −
Resp. ⟵ = −0·0005

Same lens compressed.
S.s. 5000 Exc. ⟶ +
Resp. —— = −0·0003

Exc. ⟵ −
Resp. —— − trace.

Same lens coagulated by
heat (53° C.—62° C.)
Exc. ⟶ +
Resp. —— nil.
Exc. —— −
Resp. —— nil.

Tetan. Exc. ⟶ +
Resp. —— nil.
Exc. —— −
Resp. nil.

2nd lens.
S.s. 5000 Exc. ⟶ +
Resp. ⟵ = −0·0008

Exc. ⟵ −
Resp. ⟵ = −0·0007

Tetan. Exc. ————→ +
Resp. ←———— = −0·0030

Tetan. Exc. ←———— −
Resp. ←———— = −0·0018

The lenses were now heat-coagulated in normal saline; opalescence ppeared at 52° to 55°; coagulation became complete at 62° to 72°. o trace of blaze-current could be obtained from either of the two ases after the heat-coagulation.

Exp. 28. *Lens of Rabbit's Eye.*—24 hours *post mortem.*
2nd day.
Normal current −0·0036.

		Excitation.		*Response.*
1st lens.				
(2nd day).	S.s. coil at	1000 −	=	−0·0021
		+	=	−0·0003
(3rd day.)		10000 −	=	−0·0003
		+	=	a trace −
		5000		
2nd lens. (2nd day.)				
Normal current	S.s.	1000 −	=	−0·0031
−0·0019		+	=	−0·0015

		B C A		
S.s.	5000	+ ⟶		**The first pair of excitations is effective. B is in each case the seat of an ingoing response.**
		⟶	= +0·0004	
	5000	⟵		
		⟶	= +0·0011	
	5000	+ ⟶		**The second pair of excitations is ineffective. A has presumably been exhausted by previous excitation.**
		—— nil.		
	5000	− ⟵		
		—— nil.		
Tet.	5000	*m* − ⟵ *b* + ⟶		**Response of A is, however, elicited by tetanisation in both pairs of directions.**
		⟶ ⟵	= +0·0004 −0·0011	
Tet.	5000	*m* + ⟶ *b* − ⟵		
		⟵	= −0·0011	

Exp. 30. *Rabbit's Eyes in situ.*—A similar series of trials gave in each case a small outgoing effect at each eye, presumably due to corneal response.

In the discussion, attention was drawn to the details of certain experiments—viz., Exps. 3, 7, and 20—as being in disagreement with the general rule that the lens response is negative. Exps. No. 3 and 7 were made before I had learned the importance of attending to the orientation of the lens. Exp. 20 (and Exp. 5) are instances of what, in my experience, have presented themselves as transitional types intermediate between the typical physiological response and the ordinary polarisation effects of exhausted or dead organs. I have thought it possible that certain irregularities of response occasionally met with might have been due to unavoidable injury of the *Retractor Lentis* (Campanula Halleri), described and figured by Beer.*

After Exp. No. 7, I was always careful to mark the external or corneal pole of the lens *in situ* by a speck of moist china clay, with which the clay end of electrode A was subsequently brought into contact; the opposite pole rested on electrode B.

I may take this opportunity of stating that the eyes of crabs and of lobsters gave ingoing blaze-currents (from A to B) to both directions of excitation.

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* 'Pflüger's Archiv,' vol. 58, p. 574.

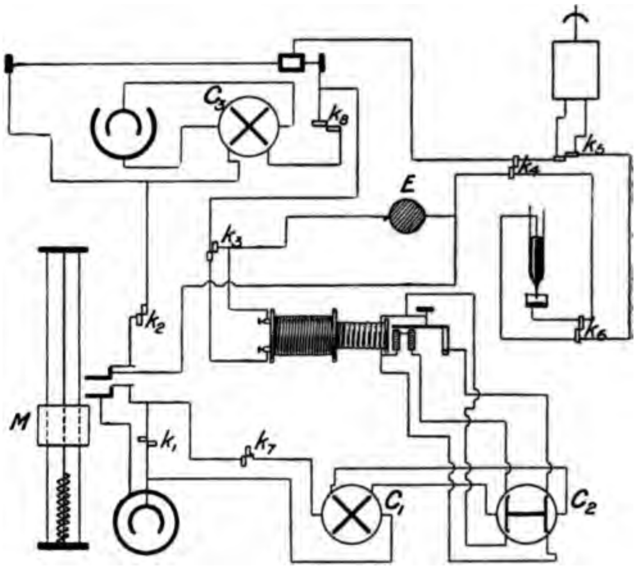
A Contribution to the Question of Blaze Currents." By Dr ARNOLD DURIG, of Vienna. Communicated by AUGUSTUS D. WALLER, M.D., F.R.S. Received November 20,—Read December 4, 1902.

(From the Oxford Physiological Laboratory.)

(Translated from the German by Miss F. Buchanan, D.Sc.)

The numerous experiments of Waller on this subject have raised e question as to whether the phenomena of the so-called "blaze rrents" are to be regarded as a proof of the persistence of life. It the purpose of the present communication to give the results of a w experiments which bear upon the subject. These relate princilly to the response of the eyeball, but also to that of a few other gans of the frog, to single induction shocks. As they are not yet efinitively concluded, I propose here to give only a short description them, and not to refer to their theoretical significance beyond ointing out that the results obtained must be taken into account efore deciding whether or not the currents in question furnish an nmistakable sign of vitality in tissues.

As regards method, the great importance of determining the exact it had broken, or made, the primary circuit. The distance between this contact and one in the primary circuit was of course so chosen that the induction current itself should produce no effect in the measuring instrument. As the sudden letting in of a resting current would have caused great disturbances in the measuring instrument when the short-circuit was broken, it was necessary to compensate this very carefully beforehand. By means of the key K_2 the compensation could be tested immediately before letting go the myograph. The myograph-stand itself was placed in an adjoining room in order that the movement of its steel rod might not affect the galvanometer. It could however be set in motion from the table on which the rest of the apparatus was placed. The secondary coil could be short-circuited by the key K_3; the measuring instrument by the key K_4, the keys K_5 and K_6 allowing of the introduction of either galvanometer or capillary electrometer to serve as such, in order that the results obtained with the one instrument might be quickly checked by the other. Although the arrangement was well adapted for concordant observations, it was defective, not only in the absence of the apparatus for recording the movements of the electrometer, but also in another more important respect. The necessity of distinguishing what is due to polarisation in the study of "blaze currents" has already been pointed out. With this in view observations ought to be made in such a way as to eliminate the effects of polarisation as much as possible, as would be accomplished by using, to produce a single exci-



ation, two almost instantaneous induction shocks, as nearly as ossible equal in strength, but opposite in direction, according to the ethod first introduced by Bernstein and by Hermann in rheotome xperiments. The use of an instantaneous make and break contact or the myograph and the introduction of a coil, suitably wound so as o avoid induction, or of an incandescent lamp, into the primary ircuit, would have been a simple means of attaining this end.

To fully acquaint the reader with what is new in the following esults, and with what is merely confirmatory of the observations of thers, that is to say of those of Waller, would make the present communication too lengthy. I will, therefore, take it for granted that Valler's researches, which form the starting-point of the subject, are nown.

For the experiments on the eye the organ was kept either in dayight or in the dark. The whole eyeball, or the special part of it that vas being investigated, lay in a slight depression of a kaolin (unpolarisble) electrode, and was connected by a thread of wick moistened in hysiological salt solution to the second electrode. The experiments vere always made on the freshly-removed eye, and so relate to Waller's irst stage only. A few observations were made on curarised frogs, he optic nerve being exposed by the removal of part of the skull, and ooped round by a piece of wick to lead it off to the galvanometer.

It is possible to so place the electrode that there shall be no resting current, and this is especially the case in the curarised animal in which the eye-circulation is maintained (Hermann, Holmgren). When the experiment lasted some time the current of rest became reversed, even when the middle of the cornea was led off from. The *response to light* is always in the positive direction, *i.e.*, in the same direction as the current of rest. This positive variation is preceded by a negative initial jerk (Vorschlag). When the light is extinguished a further positive change occurs which subsequently diminishes, at first quickly and then more slowly. Within certain limits the extent of the excursion increases with the duration and strength of the illumination. No negative initial jerk could be observed in the eye of the living frog in response to light; the increase which occurred when the illumination ceased was, however, greater and lasted for a longer time than in the excised eyeball. The return of the needle took place no more quickly than in the excised eye, notwithstanding the maintenance of the circulation and the consequent greater activity of assimilation-processes.

Excitation of the eye in the dark by light after previous excitation by electric currents usually resulted in an augmentation of the excursion, the excursions actually observed being greater than the sum of the after-effect produced in response to the induction current and the excursion which is the normal response to illumination alone. A still more striking augmentation of the effect occurred after previous faradisation (Waller). The fact that the augmentation occasionally eluded observation may be due to the circumstance that the sensitiveness of the galvanometer had often to be reduced by a tenth on account of the largeness of the excursion due to the combined effect of "blaze-currents" and light currents.

Electrical effects in response to light are given not only by the *pars optica retinæ*, but can be obtained also from the anterior part of the eyeball alone, the excursions, which could only be observed by galvanometer, being, however, very small. The possibility that the heat of the small incandescent lamp which was used for giving the light-stimulus may have been responsible for the effect must be borne in mind, and alterations which may have been produced in the muscles of the iris must also be taken into account. I am at present engaged in making the further experiments that are required with regard to this matter.

The results obtained with the eyeball in response to *excitation by electrical currents* are not in any way affected by the absence or presence of light. The currents which occur in the uninjured eye in response to such stimulation are, therefore, independent of the effects produced by light, and are to be sharply distinguished from them. (Waller.) This is confirmed by the following new observation:—

When the one leading-off electrode is moved away from the cornea towards the equator of the eyeball, not only is the difference of potential between the two poles diminished, but also the effect produced by an induction shock, which was before considerable, has now almost vanished. Instead of an excursion extending over 500 divisions it now extends over only 20 or 30, and its direction occasionally varies with that of the induction current. When the electrode is replaced on the cornea the original large deviation is again produced in response to stimulation.

When the breaking of the second contact by the myograph lets the preparation into the capillary electrometer, two different main types of response may be distinguished by the form of the observed effect. The curve is either distinctly diphasic, the first phase being negative, *i.e.*, in the opposite direction to the resting current, and the second, which is always considerably stronger, being positive and lasting for a much longer time; or a purely monophasic variation is obtained which is always positive. The negative part of the diphasic variation was so soon over that when, by means of a turn-over key, the preparation was connected with the galvanometer by hand instead of by the myograph, the most that could be observed as the last trace of the first phase was, on account of the relatively great inertia of the galvanometer, an excursion of a few and the posterior part of the eyeball are led off from, is sufficient to suggest that the real seat of origin of the "blaze currents" is to be sought for in the anterior part of the eye. Waller, who at first* regarded the "blaze currents" as having their origin in the retina, has in a subsequent† publication, stated that tissues, other than retinal, take part in their production.

Observations on Separate Parts of the Eyeball.

If the *posterior part of the eye* alone is placed on the electrodes and stimulated, the response is the same as in the whole eye when the equator is led off from, *i.e.*, only a very slight variation occurs. If the *anterior part of the eye* is investigated by itself, the effects produced are the same as when cornea and optic nerve are led off from. It was, therefore, obvious that cornea, lens, and ciliary body must next be investigated separately. One would especially expect to get electromotive effects with the *ciliary ring* on account of the attached iris and its muscles. Experiment showed that break and make induction shocks in either direction always gave deviations and excursions in a definite direction which cannot, however, be said to be positive in relation to the resting current on account of the inconstancy of the latter, which is due, no doubt, to the irregular construction of the organ. The preparation is, moreover, very perishable, and very soon gives no effect at all, or only (polarisation ?) effects, the direction of which varies with that of the exciting current.

The *lens* is much more easy to work with. From it one obtains, according to the position of the electrodes, well-marked though weak resting currents in a constant and definite direction. In response to excitation, effects in one direction (the positive) occur whatever the direction of the exciting current. They are frequently preceded by negative initial jerks when the exciting current is in the same direction, just as is the case in the whole eye. Although the "blaze currents" are in this case much stronger than they are in the ciliary body, those of the two together are not nearly sufficient to account for the currents in the whole eyeball. The only remaining part of the eye in which to seek for the principal seat of origin of those large differences of potential, which occur in the response of the whole eye, was now *per exclusionem* the *cornea*, and, according to expectation, this was found by experiment to give strong electromotive effects. The preparation was made by cutting into the eyeball at the edge of the cornea with scissors, and then cutting right round the limbus. The inner surface of the removed cornea was placed on a little projection of the kaolin of the one electrode, the outer surface was connected by a thick piece of wick with the other. In response to every stimulation, large "blaze currents" occurred, which in all respects resembled those of the whole eyeball, even in their intensity, so that one would not have known that the experiment was not being made with the whole eye unless one had seen the preparation. The resting current was from the inner to the outer surface; each induction shock, in whichever direction, produced an effect in the same direction as the resting current. When the excitation was also in the same direction as the resting current, this (outgoing, Transl.) response was preceded by a well-marked initial effect in the opposite direction.

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* Waller, "On the Retinal Currents of the Frog's Eye, excited by Light and excited Electrically," 'Phil. Trans.,' B, vol. 193, p. 123.

† "On the 'Blaze Currents' of the Frog's Eyeball," 'Phil. Trans.,' B, vol. 194, p. 183.

According to these observations, the principal place in which the "blaze currents" of the eyeball originate appears to be the cornea, while the ciliary ring and lens contribute thereto. The retina plays quite a subsidiary part in their production. The results of these experiments on the cornea give support to the analogies between skin and cornea in their behaviour to excitation, to which Waller, by comparing the eyeball and skin currents, has already drawn attention, although it must not be forgotten that, owing to the difficulty in preparing it, the cornea is a much damaged tissue.

The fact that the cornea, an epithelium which is free from mucous their absence as a sign of its cessation. For the solution of this question, a few experiments were undertaken with other organs of the frog, namely, liver, kidney, and ovary of freshly-killed animals. With such specifically-surviving organs as liver and kidney one would have especially expected to find the currents which are supposed to be a sign of life.

Experiments on Liver, Ovary and Kidney.

Only one out of all the experiments made gave a result which could in any way be compared with that obtained from the eye. The other preparations all gave electromotive effects which were often very considerable, but which cannot be otherwise regarded than as polarisation effects, since they varied with the direction of the exciting current and were always opposed to it. The one experiment which formed the exception can hardly be said to agree with the observations on the eye, since, although the excursions were certainly always in one direction, they varied enormously in extent, according to the direction of the exciting current, the proportion being sometimes as much as 8 to 1. Faradisation of the organs produced no result at all when the coil was so arranged that the make and break shocks were of equal intensity.

It seems to me no longer possible, after these observations, to regard the appearance of "blaze currents" as a specific property of living tissue. It is much more probable that they are to be considered as special manifestations of certain epithelial tissues.

Neither can the presence of exclusively polarisation effects be taken as the sign of the death of a tissue, since these may occur alone in a very pronounced manner in living organs, and in a few organs may even represent the regular and typical effect to stimulation.

In conclusion, it is my pleasant duty to thank Professor Gotch, in whose laboratory these experiments have been carried out, for the facilities afforded me.

'The Specific Heats of Metals and the Relation of Specific Heat to Atomic Weight. Part II." By W. A. Tilden, D.Sc., F.R.S., Professor of Chemistry in the Royal College of Science, London.

Received December 8,—Read December 11, 1902.

(Abstract.)

The following values have been obtained for the mean specific heats f pure aluminium, nickel, cobalt, silver, and platinum, within the everal limits of temperature indicated:—

Centigrade.	Aluminium.	Nickel.	Cobalt.	Silver.	Platinum.
−182° to +15°	0·1677	0·0838	0·0822	0·0519	0·0292
− 78 „ +15	0·1984	0·0975	0·0939	0·0550	—
+ 15 „ 100	—	0·1084	0·1030	0·0558	0·0315
15 „ 185	0·2189	0·1101	0·1047	0·0561	—
15 „ 335	0·2247	—	—	—	—
15 „ 350	—	0·1186	0·1087	0·0576	—
15 „ 415	—	0·1227	—	—	—
15 „ 435	0·2356	0·1240	0·1147	0·0581	0·0338
15 „ 550	—	0·1240	0·1209	—	—

Range of temperature.	NiS.	Ag_2S.
− 182° to + 15°	0·0972	0·0568
15° „ 100°	0·1248	0·0737
15° „ 324°	0·1333	0·0903

The mean value for the specific heat of silver sulphide is less than that for nickel sulphide throughout, but little can be deduced from the results till the influence of temperature on the specific heat of sulphur is known.

"Preliminary Note on the Relationships between Sun-spots and Terrestrial Magnetism."

By C. CHREE, Sc.D., LL.D., F.R.S.

Received December 18, 1902,—Read January 22, 1903.

(From the National Physical Laboratory.)

I have been engaged during the last two years on an analysis of the magnetic results obtained at Kew Observatory (now the National Physical Laboratory), during an 11-year period, 1890 to 1900. The work has been much interrupted, and is still incomplete. Amongst the points dealt with is the inter-relationship between sun-spot frequency and magnetic phenomena, and, as this has recently been engaging attention elsewhere, I have decided to put certain of my results on record at once. It has long been known from the researches of Balfour Stewart, Ellis, and others, that there is a close connection between the times of occurrence of greatest sun-spot frequency and largest amplitude of the diurnal inequality of magnetic declination and horizontal force. I have investigated whether the numerical relationship between the phenomena can be adequately represented mathematically in a simple way.

A convenient basis for the investigation was presented by the publication by Professor Cleveland Abbe in the 'U.S. Monthly Weather Review,' for November, 1901, of a table of sun-spot frequencies as calculated by Wolf and Wolfer for a very long series of years. After I had carried out all the calculations, Wolfer himself published a similar table* embodying his latest corrections. The differences from Abbe's table are trifling, and mainly confined to two years (1891 and 1892). I judged it best, however, to revise the whole of my arithmetic, so as to employ Wolfer's own most approved figures. In the following remarks S represents Wolfer's value for the sun-spot frequency. The above-mentioned table gives the mean S for each month and for each year.

The magnetic quantity selected for comparison is the mean monthly "range," meaning thereby the difference between the greatest and ast of the twenty-four hourly values in the mean diurnal inequality r the month in question, based on the five *quiet* days selected for the onth by the Astronomer Royal. Calling this quantity R for any articular magnetic element, I tentatively assumed

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* 'Met. Zeitschrift,' May, 1902 p. 195.

$$R = a + bS \ldots\ldots\ldots\ldots\ldots\ldots \quad (1),$$

ith a and b constants. I grouped together the 11 Januarys, the 11 ebruarys, and so on, of the 11-year period, and determined a and b y least squares for each of the resulting 12 groups. There being nly 11 years' data, the calculated values doubtless are appreciably ffected by quasi-accidental irregularities, but there is so striking a esemblance between the more conspicuous features of the results ound for the declination, inclination and horizontal force as to stify the conclusion that the phenomena are *bonâ fide*. Full partiulars will be given later. At present it will suffice to record the mean alues found for the a and b of the formula for three groups of months -viz. :—

Winter, comprising November to February,
Equinox ,, March, April, September, October, and
Summer ,, May to August.

The results are as follows :—

In obtaining these figures I have retained a figure in the value of b beyond that recorded in Table I.

Tables I and II will suffice to bring out one of the most important points established, viz., that b is certainly different from one month to another, and is, for all the elements except the vertical force, decidedly larger at the equinoxes (more especially it would appear at the spring equinox) than at other seasons. This means that the equinoxes are the seasons at which the amplitude of the diurnal inequality, when considered *absolutely*, is most dependent on the sun-spot frequency. When we take into account, however, the difference between the ranges of the diurnal inequalities at different seasons of the year, we find that winter is the season when sun-spot frequency is *relatively* most important. This will be recognised on reference to Table III, remembering that a represents the range corresponding to a total absence of sun-spots, while $a + 41{\cdot}7\,b$ is the range corresponding to a sun-spot frequency of 41·7, this being, as already mentioned, Wolfer's mean value for the 11 years in question.

Table III.

Values of $41{\cdot}7b \div a$.

	Declination.	Inclination.	Horizontal force.	Vertical force.
Winter	0·42	0·69	0·60	0·19
Equinox	0·27	0·49	0·39	0·06
Summer	0·20	0·35	0·26	0·07

Table III serves also to bring out another important result, viz., that the influence of sun-spot frequency on the amplitude of the diurnal inequality is very much less for the vertical force than for the three other elements considered.

A recent interesting paper by Rajna* shows that the idea of a linear relationship between diurnal magnetic range and sun-spot frequency has already been applied by at least two previous investigators, Rajna and Wolfer. They seem, however, to have applied it only to mean annual values, and to have considered declination only. Rajna, dealing with declination data, observed at Milan over the long period 1836 to 1901, applies a formula of type (1) to what he calls the "medie annuali dell' escursione diurna."

The value he finds for b is 0′·047. He mentions that in an earlier similar investigation, including declination data from several stations, Wolfer obtained the value 0′·040.

I am uncertain as to the precise meaning of Rajna's "medie annuali," but it certainly is not quite the same thing as the mean range in Table I, so that the results are not absolutely comparable.

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* 'Rendiconti del R. Ist. Lomb.,' Serie II, vol. 35 1902.

Another recent and able paper bearing on the subject appears in the st published volume of the French Bureau Météorologique, which is just come into my hands. The author, Mr. Alfred Angot, has nticipated me in applying a formula of type (1) to the individual onths of the year; but he treats of the amplitude, not of the diurnal nge as a whole, but of that of the coefficients of the several terms of e Fourier's series into which the diurnal inequality can be analysed. he paper treats only of the declination—dealing with data from rdinary days at Parc St.-Maur, Greenwich and Batavia—but the uthor expresses his intention of considering in the future the orizontal force.

A special feature of the present investigation is that the magnetic ata are derived exclusively from magnetic *quiet* days. This suggests once a query and a criticism, a query as to why one did not employ orresponding sun-spot data confined to the magnetically quiet days, criticism that as the two sets of data employed do not absolutely orrespond, the comparison actually made may be misleading.

As to the query: Wolfer, it is true, publishes at regular intervals in e 'Met. Zeitschrift' *provisional* sun-spot frequencies for each day. hese figures are, however, presumably inferior in certainty to the final gures he has embodied in his table after consulting all available urces of information. The vital consideration, however, is that at

"Characteristics of Electric Earth-current Disturbances, and their Origin."

By J. E. TAYLOR. Communicated by Sir OLIVER LODGE, F.R.S.

Received December 16, 1902,—Read January 22, 1903.

The following notes refer to effects which appear to have a distinct connection with the so-called "ionisation" of the upper regions of the atmosphere by radiations from the sun, and which have repeatedly attracted my attention during the course of recent experiments in wireless telegraphy for the British Postal Telegraphs.

In the electronic theory of the causes producing the aurora borealis, it is assumed that by the deflection of the course of the flying ions or electrons towards the poles, due to the earth's magnetic field, a concentration results in those neighbourhoods, giving rise to the phenomenon.

The effects classed by telegraph engineers as earth-currents have also, apparently, a direct connection with the ionisation of the atmosphere. As is well known, these are at times, particularly when auroral displays are in evidence, so strongly pronounced as to interfere more or less with ordinary telegraphic working on earthed circuits.

In special cases, where sensitive apparatus is used, they are, every day, sufficiently pronounced to cause disturbance, for some hours at least, even under normal conditions. They have been found to be particularly troublesome in the Post-office system of wireless telegraphy, in which a sensitive telephone receiver is connected in a low resistance circuit earthed in the sea at both ends.

To enumerate in a systematic manner the various investigations which have been made from time to time on the subject of earth-currents would involve a lengthy paper; but only the more prominent features which have forced themselves on my observation will here be briefly summarised.

The disturbances evidence themselves by producing various characteristic noises in the telephone receiver. They have not been confounded with ordinary telegraphic or other inductive disturbances, as they appear in circuits far removed from any such source of affection. In these latitudes they are always stronger and of more frequent occurrence in summer than in winter. They are daily in evidence for a few hours at or about the time of sunset, *i.e.*, whilst daylight is fading.

In general they do not evidence themselves to any great extent during broad daylight, but are readily precipitated by atmospheric electrical effects or any tendency to thunderstorms, and rarely, if ever, fail to herald the approach of a storm or gale.

The characteristic noises produced may be divided into five classes sembling—

(i) Uniform flowing or rushing of water: this is usually a day-me disturbance, and is occasionally of considerable vigour.

(ii) Intermittent crackling: an accompaniment of other disturbances.

(iii) Bubbling and boiling of water: the usual form of nightfall sturbance, but also frequently occurring in the daytime.

(iv) Rocket disturbances. These are peculiar and characteristic, ving some resemblance to the sound produced by a rocket rising the air. They commence with a shrill whistle and die away in a te of diminishing pitch. They vary in intensity, but always have similar duration of from 2 to 4 seconds; are freely heard at night, d only occasionally during the day.

(v) Disturbances due to high frequency effects, inaudible on the lephone, but evidenced on the coherer, magnetic detector, or other rm of Hertzian receiver.

These various disturbances were, for some time, very puzzling to e; but on perusing Professor J. J. Thomson's paper, read at the yal Institution on 19th April, 1901, it speedily appeared highly obable that they were due to electrical effects produced in the mosphere by the ionisation caused by solar radiations and the action on this ionisation by electric stresses in the atmosphere.

the atmosphere and the ionisation produced by the sun suggests the source of the other daytime disturbances referred to. It appears highly probable that they are the accompaniment of the clearing up process. Effects analogous to, if not actually, electric currents are doubtless produced in the atmosphere, which are induced in the sea and collected by the circuit. On the other hand, the nightfall disturbances are probably due to normal clearing up processes, revealed when the air becomes sufficiently non-conducting to act no longer as a screen. These suggestions, though by no means complete, are submitted for what they may be worth.

It is probable also that the diurnal variations of the earth's magnetic field are influenced by the same causes.

One more point. The periods of maximum disturbances, experienced on the earthed circuits referred to, appear to coincide with periods of maximum atmospheric disturbances on the newer Hertzian system of wireless telegraphy, and indicate the same source of trouble. Further, I would suggest that we have here a clue to the true explanation of the greater night-time efficiency in signalling observed by Mr. Marconi in recent experiments. With ionised air the electric waves will be partly broken up and absorbed, with consequent abstraction of energy from the transmission. At night, when the ionisation is cleared up, the strength of the radiated waves will be sustained.

Some interesting investigations by the aid of sounds produced, in a telephone, by the passage of electrical currents through rarified gases can no doubt be carried out. Professor Righi has already made some observations in this connection, but much more can yet be done.

"Solar Eclipse of 1900, May 28.—General Discussion of Spectroscopic Results." By J. Evershed, F.R.A.S. Communicated by the Joint Permanent Eclipse Committee. Received December 17, 1902,—Read January 22, 1903.

(Abstract.)

In a general way the conclusions arrived at from the discussion of the spectra obtained in 1898 are amply confirmed and extended by the present results. It is now shown that every strong dark line of the solar spectrum exceeding Rowland's intensity 7 is found in these spectra as a bright line; and the great majority of the bright lines of the flash spectrum, excluding hydrogen and helium lines, coincide with dark lines of intensity not less than 3.

Most of the bright arcs of the flash spectrum are well-defined narrow lines admitting of considerable accuracy in the measures, and the present determinations of wave-length indicate that the coincidence of the bright lines with the dark lines is exact within ·05 t.m. for all the well-defined lines.

As regards the relative intensities of the lines of any one element in the flash and Fraunhofer spectra, my previous results require modification and extension as follows: The relative intensities of isolated lines of an element in the flash spectrum are in general, but not exact, agreement with those of the same element in the solar spectrum, and those lines which are exceptionally strong in the flash are in most cases lines which are enhanced in the spark spectrum of the element.

All of the more prominent enhanced lines of iron and titanium, as determined by Sir Norman Lockyer, are found to coincide with strong lines in the flash, but owing to the compound nature of some of the lines, it is not certain that all of these have abnormal intensities in the flash.

There is no evidence of differences in the relative intensities of the lines of an element in the higher or lower regions of the flash layer, and the enhanced lines appear to predominate throughout the entire depth of the radiating stratum. The enhanced lines are equally prominent in the polar regions and in low latitudes, and the flash spectrum generally is now found to be the same in all latitudes and shows no essential change after an interval of five years.

An explanation of the abnormal intensities of the enhanced lines in the flash spectrum is now offered, which depends on the assumption of a continuous circulation of the solar gases in a radial direction; the highly heated ascending gases giving the predominant features to the flash spectrum, whilst the cooler more diffused gases, slowly ng, determine the character of the absorption spectrum.

The entire chromosphere is supposed to consist of innumerable small eruptions or jets of highly-heated gases similar to the so-called "metallic" prominences, which are only the more pronounced manifestations of the same eruptive agencies.

Evidence for this is found in the characteristic features of the chromosphere, and in the detailed structure of many of the Fraunhofer lines, which show wide emission lines underlying the narrow absorption lines. These ill-defined bright lines in the normal solar spectrum are distinctly displaced towards the violet, indicating a strong uprush of the hotter gases, whilst the narrow absorption lines are almost in their normal positions, and appear to indicate a slow and uniform descent of the absorbing gases.

The final conclusion is that the flash spectrum represents the emission of both ascending and descending gases, whilst the Fraunhofer spectrum represents the absorption of the descending gases only.

"On the Electrodynamic and Thermal Relations of Energy of Magnetisation." By J. LARMOR, M.A., D.Sc., Sec. R.S. Received January 2,—Read January 22, 1903.

1. There appears to be still some uncertainty as to the principles on which the energy of magnetised iron is to be estimated, and the extent to which that energy is electrodynamically effective. The following considerations are submitted as a contribution towards definite theoretical views.

The electrokinetic energy of a system of electric currents $\iota_1, \iota_2, \ldots$, flowing in complete linear circuits in free aether, is known to be

$$\tfrac{1}{2}(\iota_1 N_1 + \iota_2 N_2 + \ldots);$$

wherein N_1 is the number of tubes of the magnetic force (α, β, γ) that thread the circuit ι_1, and is thus equal to $\int(l\alpha + m\beta + n\gamma)dS$ extended over any barrier surface S which blocks that circuit, (α, β, γ) being circuital (*i.e.*, a stream vector) so that all such barriers give the same result. As under steady circumstances (α, β, γ) is also derivable from a magnetic potential V, which has a cyclic constant $4\pi\iota$ with regard to each current, this energy assumes the form

$$\frac{1}{8\pi}\Sigma\int V\left(l\frac{\partial V}{\partial x} + m\frac{\partial V}{\partial y} + n\frac{\partial V}{\partial z}\right)dS,$$

in which the integrals are now extended over both faces of each arrier surface. This is equal by Green's theorem to the volume-ntegral

$$\frac{1}{8\pi}\int(\alpha^2+\beta^2+\gamma^2)\,d\tau$$

xtended throughout all space. This latter integral is in fact taken n most forms of Maxwell's theory to represent the actual distribution, n all circumstances whether steady or not,* of the electrokinetic nergy among the elements of volume of the aether, in which it is supposed to reside as kinetic energy.

2. The most definite and consistent way to treat magnetism and ts energy is to consider it as consisting in molecular electric currents; o that in magnetic media we have the ordinary finite currents, ombined with molecular currents so numerous and irregularly rientated that we can only average them up into so much polarisation er unit volume of the space they occupy. So far in fact as the tter currents are concerned, the only energy that need or can ccupy our attention is that connected with some regularity in their rientation, *i.e.*, with magnetisation, the remaining irregular part eing classed with heat. If there were no such molecular currents, he magnetic force (α, β, γ) in the aether would in steady fields be erived from a potential cyclic only with regard to the definite number

If we compare this result with the customary magnetic vectors of Kelvin and Maxwell, it appears that (α, β, γ) must represent the "induction," and so will hereafter be denoted, after Maxwell, by (a, b, c). The new vector, which has a potential cyclic with respect to the finite currents only, represents the "force," and will hereafter be denoted by (α, β, γ), whose significance is thus changed from henceforth. The "induction" on the other hand has not necessarily a potential, but is, by the constitution of the free aether, always circuital; that is, it satisfies the condition of streaming flow

$$\frac{\partial a}{\partial x} + \frac{\partial b}{\partial y} + \frac{\partial c}{\partial z} = 0.$$

The expression for the energy now includes terms

$$\tfrac{1}{2}(\iota_1 N_1 + \iota_2 N_2 + \ldots)$$

for the ordinary currents $\iota_1, \iota_2, \ldots$, where $N_1, N_2, \ldots$ are the fluxes, *of magnetic induction*, through their circuits; this transforms as usual into

$$\frac{1}{8\pi} \Sigma \int V(la + mb + nc)\, dS$$

over both faces of each barrier, which by Green's theorem is equal to

$$\frac{1}{8\pi} \int (a\alpha + b\beta + c\gamma)\, d\tau \quad \text{(i)}$$

extended throughout all space. But there are also terms

$$\tfrac{1}{2}(\iota_1' N_1' + \iota_2' N_2' + \ldots)$$

for the molecular currents; now taking N' to be the cross-section of the circuit multiplied by the component of the averaged induction normal to its plane, and remembering that ι' multiplied by this cross-section is the magnetic moment of this molecular current, it appears that $\iota' N'$ is equal to the magnetic induction multiplied by the component of the magnetic moment in its direction, and therefore $\tfrac{1}{2}\Sigma\iota' N'$ is equal to

$$\tfrac{1}{2} \int (Aa + Bb + Cc)\, d\tau.$$

Thus the magnetic circuits add to the energy the amount*

$$\tfrac{1}{2} \int (A\alpha + B\beta + C\gamma)\, d\tau \quad \text{(ii)}$$

together with

$$2\pi \int (A^2 + B^2 + C^2)\, d\tau \quad \text{(iii)}$$

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* [These energies as here determined are *kinetic*; if they are (as is customary) to be considered as *potential*, their signs must be changed. *Cf.* 'Phil. Trans.,' A, 1894, p. 806.]

The formula (i) is usually taken, after Maxwell's example, to present the energy of the electrokinetic field. It here appears that represents only the part of the energy that is concerned with the rrents, arising from their mutual interactions and the interactions of e magnets with them: that there exists *in addition* a quantity (ii) hich is that taken by Maxwell as the energy of magnetisation in e field (α, β, γ), and also a quantity (iii), which is purely local and nstitutive, of the same general type as energy of crystallisation. he question arises whether (iii) is a part of the intrinsic energy of agnetisation of different kind from (ii), in that it cannot even rtially emerge as mechanical work, or on the contrary the usual rmula (ii) must be amended. See §§ 5, 8. In any case the ynamics of the field of currents (when there are no irreversible atures) involves only that part of the energy function in which the rrents operate, thus excluding both (ii) and (iii).

3. The simplest example is that of a coil of n turns carrying a rrent ι, wound uniformly on a narrow iron ring-core, of crossction S and length l. On the present basis the energy is made up an electrodynamic part $\frac{1}{2}\iota n\mathfrak{B}\mathrm{S}$ and a magnetic part $\frac{1}{2}\mathfrak{H}\mathfrak{B}\mathrm{S}l$; as $n\iota = \mathfrak{H}l$ by the Amperean circuital law, these parts are

force its way, to be expelled again by the elasticity of its walls when the stream begins to set in the reverse direction. This increase of kinetic pressure on the turbine roughly represents the electromotive pressure on a motor due to the increased magnetic flux, and the energy spent in expanding the reservoir as it fills up represents the energy of magnetisation of the iron. If things were perfectly reversible in the reservoir, that is if the iron were perfectly soft, the latter energy would rise and fall concomitantly with the alternations of pressure on the motor, but of course if its temperature remained constant it would contribute nothing to the energy driving the motor, which must be introduced into the system from an extraneous source. But if there are frictional resistances involved in filling the reservoir, the operations will not be perfectly reversible, and mechanical energy will be lost in it by conversion into heat; and moreover on account of the phase of its changes getting out of step—still more by permanent delays such as are classed under hysteresis—it will operate less efficiently in directing the stream of energy towards the turbine. Both these statements have analogical application to the iron in a magnetic circuit.

An example is provided by the ring-coil aforesaid. Suppose that when the current has ceased in the coil the core retains permanent magnetism, its energy being the latter term in the formula above. This corresponds to the reservoir becoming temporarily choked, so that it retains its contents after the pressure that drove the liquid into it has been removed. The question arises whether this retained energy is available for mechanical work. The present aspect of the matter appears to lead to the conclusion (Lord Rayleigh's) that it will not be available to any considerable extent unless its pressure in the reservoir is considerable, that is, in the magnetic case, unless the iron is not very receptive of magnetisation.

The paradox that energy of residual magnetism, which is outside the electrokinetic system, can on running down affect that system, shows that the circumstances are more general than an analogy of a pure dynamical system of finite number of degrees of freedom can illustrate. In fact the equations of dynamics imply permanent structure of the system; whereas in Professor Ewing's illustrative model of paramagnetisation, when the displacement is great enough the structure changes by the component magnets toppling over,* and after the general disturbance thus set up has subsided with irrecoverable loss of energy into heat, there remains a new structure to deal with. The only way to estimate the available part that may be latent in the great store of energy of residual magnetism of an iron core is thus by the empirical process of detailed experiment. Lord Rayleigh has inferred rom the form of the curve of hysteresis for retentive iron in igh fields that the fraction that is directly available at the actual emperature must always be small, and he supports the inference by onsiderations of the nature of the above analogy; in the absence of ysteresis there would be no such direct availability. He derives the ractical result that a complete magnetic circuit is deleterious for nduction coils in which length of spark is the *desideratum*, the increased otal induction attained inside the ring-core being more then neutraised by the diminished promptness of magnetic reversal.

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* The effective susceptibility dI/dH becoming enormous in the steep part of the characteristic curve.

In fact, if the core, laminated so as to have merely negligible conuctivity, is surrounded by a perfectly conducting coil or sheath, and s permanent magnetism is removed at constant rate $-d\mathrm{I}/dt$ by an leal process applied to it, the intensity of induction in the core vill diminish at the rate $-4\pi d\mathrm{I}/dt$; and this defect of induction nust be made good by the influence of the current thereby induced in he sheath, as otherwise there would be a finite electromotive power in , which is impossible on account of its perfect conductivity. This estored induction is of the form $\mathrm{H}'+4\pi\mathrm{I}'$, where I' is the magnetism nduced by the force H' due to the induced current; thus

$$\frac{d}{\ }(\mathrm{H}'+4\pi\mathrm{I}')+4\pi\frac{d\mathrm{I}}{\ } = 0.$$

derived from purely thermal sources; and the following thermodynamic argument* will strengthen this conclusion.

If the value of the magnetic susceptibility κ for any material is a function of the temperature, we can perform a Carnot reversible cycle by moving a small portion (say a sphere) of the substance in the permanent field of a system of magnets supposed held rigidly magnetised by constraints. We can move it into a stronger region H_2 of this field, of varying strength H, maintaining it at the temperature θ by a supply of heat from outside bodies at that temperature; we can then move it on further, having stopped the supply of heat, until its temperature becomes $\theta - \delta\theta$; we can move it back again isothermally by aid of a sink of heat at this temperature until the stage H_1 is reached, when further progress back adiabatically will restore it to its original condition. If κ is a function only of the strength of field and of the temperature, this cycle will be reversible. If E is the heat-energy supplied at temperature θ, and W is the work done on the sphere by external bodies in the cycle, the principle of Carnot gives the relation

$$\frac{E}{\theta} = \frac{W}{\delta\theta}.$$

Now

$$W = -\frac{d}{d\theta}\left(\tfrac{1}{2}I_2H_2 - \tfrac{1}{2}I_1H_1\right)\delta\theta$$

$$= -\tfrac{1}{2}\frac{d\kappa}{d\theta}\left(H_2^2 - H_1^2\right)\delta\theta, \text{ if } \kappa \text{ is small,}\dagger$$

when the cycle is taken such that the change of H along the adiabatic art of the path is negligible compound with that along the isothermal art. Thus

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* This theoretical deduction of Curie's law has been already given substantially in 'Phil. Trans.,' A, vol. 190, 1897, p. 287.

The theory of diamagnetism, which assigns it to modification of conformation in the individual molecule by the inducing field rather than to average spacial orientation of the crowd of molecules, leads to a non-thermal origin as regards that part. The analogous question (*loc. cit.*) as to whether dielectric polarisation is mainly an affair of orientation of unaltered molecules like paramagnetism, or one of polarity due to internal deformation of the molecule like diamagnetism, is now answered by the experiments of J. Curie and Compan ('Comptes Rendus,' June 2, 1902). It appears that the dielectric coefficient of glass, for rapid changes, *diminishes*, but not very quickly, with fall of temperature, and that at temperatures below −70° C. duration of charge ceases to have influence on its value. The electric excitation is thus analogous to diamagnetism and has no thermal bearing, its energy being self-contained in the molecule; the signs of the susceptibilities in the two cases are different, because the one is of static, the other of kinetic character. The sharpness of the Zeeman magneto-optic effect has already led ('Aether and Matter,' 1900, p. 351) in this direction, for it requires that the electric polarisation in the molecule shall be of isotropic type, so that there may be no axis of maximum susceptibility.

† This restriction is not necessary for the final result; if κ is not small, W and E have both to be multiplied by the same factor.

$$E = -\tfrac{1}{2}(H_2^2 - H_1^2)\,\theta\,\frac{d\kappa}{d\theta}.$$

ow the experiments of Curie on the relation of κ to θ in weakly *aramagnetic* materials make κ vary inversely as θ; and this result has ore recently been verified down to very low temperatures by Dewar nd Fleming. This gives

$$E = \tfrac{1}{2}\kappa(H_2^2 - H_1^2).$$

hus the movement of the magnetisable material at uniform temperaure is accompanied by a supply to it of heat, equal to the mechanical ork done by it owing to the attraction of the field; and this heat s just what is wanted to be transformed into the additional energy of ntrinsic magnetisation (ii) of § 2. It is to be observed that in the ctual experiments κ was small, and the other part (iii) of this energy herefore negligible: so that no conclusion as to the extent to which ts source is thermal can be derived from Curie's law.

6. The uncertainties of § 4 do not of course affect the estimation f the loss of motive power arising from cyclic magnetic hysteresis,

the integral being taken along the path. If (A, B, C) is a function of (α, β, γ), that is if the magnetism is in part thoroughly permanent, and in part induced without hysteresis, so that the operation is reversible, this work must vanish for a complete cycle; otherwise energy would inevitably be created either in the direct path or else in the reversed one of the complete system of which $\delta\tau$ is a part. Thus the negation of perpetual motion in that case demands that

$$\mathrm{A}d\alpha + \mathrm{B}d\beta + \mathrm{C}d\gamma = d\phi,$$

where ϕ is a function of (α, β, γ), involving only even powers, and practically quadratic for small fields. Its coefficients are then the six magnetic constants for general aeolotropic material, no rotational quality in the magnetisation being thus allowable by the doctrine of energy. But if there is hysteresis, so that the cycle is not reversible,

$$-\delta\tau \int (\mathrm{A}d\alpha + \mathrm{B}d\beta + \mathrm{C}d\gamma),$$

or in vector product form $-\delta\tau \int \mathfrak{F}\, d\mathfrak{I}$, represents negative mechanical work done, or energy degraded, in the cycle.

In addition to this energy concerned with attraction, the external field expends energy in polarising or orientating the individual molecules against the internal forces of the medium, of aggregate amount

$$\delta\tau \int (\alpha d\mathrm{A} + \beta d\mathrm{B} + \gamma d\mathrm{C}).$$

In any case, whatever the hysteresis, the sum of this second part and the first reversed is integrable independently of the path, giving

$$\delta\tau \mid \mathrm{A}\alpha + \mathrm{B}\beta + \mathrm{C}\gamma \mid,$$

namely, the change in the total energy in the element, thus vanishing for a cycle which restores things to their original state, as it ought to do The latter part is purely internal, and of merely thermal value as in § 5. The former part represents the averaged waste of direct mechanical energy in moving the iron armature through the cycle, and accounts for the heat thus evolved. It is the expression of Warburg and of Ewing for magneto-hysteretic waste of mechanical energy in driving electric engines; for a portion of a cycle it represents work partly degraded and partly stored magnetically.

7. Reverting to § 5, we may profitably illustrate by working out into detail a suggestion of Lord Rayleigh (*loc. cit.*). Consider a ring-coil of n turns with a flexible open core of soft iron of length l and cross-section S, whose flat ends are bent round until they face each other at a distance small compared with the diameter of section. We can apply Hopkinson's theory of the open magnetic circuit to trace the

97 was observed at 2 million periods per second. It is possible, erefore, that some residual charge has already come out at 2 million riods per second.

When the glycerine was warmed up to atmospheric temperature 10° C., still remained *solid*, and its dielectric properties were examined at is temperature. The resistance of the condenser was 216000 ohms, hether the time of contact was 0·00002 or 0·011 second, and this is out $3\frac{1}{4}$ times the resistance at 10° C. when the glycerine was in the *uid* state. No residual charge comes out between the above times. 2 million periods per second the specific capacity of this solid ycerine at 10° C. is 6·67. By the method described previously* e specific capacity is 16 at 50 periods per second. The specific pacity in the liquid and solid states at 10° C. varies approximately the conductivity for long times. The residual charge comes out at nes less than 0·00002 second, whether the glycerine be liquid or solid 10° C.

Finally, the condenser was frozen in carbonic acid snow in ether, and the conductivity at −81° C. is shown by the curve No. 2 in the

the conductivity falls and then rises, as the temperature is varied from +13° to −59° C. when freezing from the liquid to the solid state. An analogous effect has been observed in soda-lime glass.*

When the time of contact is 0·006 second, all the points in the table lie well on the same curve between −26° and −81° C. It is when freezing from the liquid to the solid states that such serious changes occur at the short times.

Time of contact in seconds after application of force.	Conductivity in 10^{-6} ohms^{-1}.						
	Freezing from liquid to solid state.			Freezing from solid state.			
	+13° C.	−26° C.	−59° C.	+12° C.	−48° C.	−75° C.	−81° C.
0·00002	15	1·72	8·03	4·63	0·383	0·191	0·197
0·00017	..	..	..	..	..	..	0·0301
0·00035	..	..	0·172	..	..	..	0·0201
0·00099	..	..	0·0708	..	..	..	0·0120
0·0028	..	..	0·0481	..	..	..	0·0109
0·0060	..	..	0·0319	..	0·134	0·012	0·0092
0·0090	15	0·67	0·0287	..			
0·011	..	..	0·0249	4·63			
0·018	..	..	0·0160				
			Curve No. 1				Curve No. 2

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* Hopkinson and Wilson, 'Phil. Trans.,' A, vol. 189 (1897), pp. 109—136.

'he Relation between Solar Prominences and Terrestrial Magnetism."

By Sir NORMAN LOCKYER, K.C.B., F.R.S., and WILLIAM J. S. LOCKYER, M.A., Ph.D., F.R.A.S. Received January 14,—Read January 29, 1903.

[PLATES 4 AND 5.]

It has been stated in a previous communication* that a preliminary luction of the Roman observations of prominences, observed on the n's limb by Tacchini, indicated that, in addition to main epochs of axima and minima of prominences coinciding in time with those of e maxima and minima of the total spotted area, there are also ominent subsidiary maxima and minima.

One of us has pointed out in a recent communication to the cadémie des Sciences † that a comparison of the frequency of ominences visible in each solar latitude with the frequency of the ost intense magnetic storms, indicated that (*a*) magnetic storms ssed as "great" by Ellis, and the greatest prominence activity near e poles of the sun occurred at the same time; and (*b*) that the curve general magnetic activity was nearly the same as that of the

In the curves accompanying the present communication (Plate 4) the above-mentioned set, except those for 80°—90° north and south, have been grouped in pairs, thus representing the percentage frequency of prominences in each hemisphere for zones of 20° of latitude, 0°—20°, 20°—40°, &c., since it was found that this reduction could be made without losing any of the characteristic variations.

An examination of these curves shows that they differ very considerably one from the other as we proceed from the equatorial to the polar zones. Generally speaking the curves representing the variations for each of the zones, 0°—20° north and south, conform with the sun-spot curve; that is, the maxima and minima occur at about the epochs of sun-spot maxima and minima. Those for the two zones, 20°—40°, in both hemispheres conform also in the main to the general sun-spot curve, but in addition they display subsidiary maxima or changes of curvature superimposed on the main curve.

The curves for the two zones, 40°—60° north and south, have, on the other hand, hardly any likeness to the sun-spot curve, but are made up of a series of prominent maxima representing special outbursts of prominence activity.

Passing to the curves corresponding to the next zones, *i.e.*, 60°—80° north and south, these indicate two prominent outbursts lasting for a short period, showing that this region of the sun is, as a rule, practically free from prominence activity; in the remaining zones, 80°—90° north and south, the variation is small, and is a faint echo of the condition of affairs in the neighbouring zone 60°—80°.

The Magnetic Curves.

The data regarding the magnetic phenomena employed in this com- [illegible] are those brought together by Mr. William Ellis, in two [illegible] papers on magnetic phenomena.*

[illegible] y take the opportunity here of thanking Mr. Ellis for kindly [illegible] ting to us a continuation of the data published in these two [illegible] h information he has brought down to the year 1899.

[illegible] of magnetic phenomena were there dealt with, namely, [illegible] rom year to year of the diurnal range of the declina- [illegible] ntal force, and magnetic disturbances.

[illegible] the former, Mr. Ellis has shown† that the curves indi- [illegible] variations are very similar to that of the general sun-spot [illegible] tact, the curves were found to be almost identical in all th[illegible] aller irregularities.

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[*] 'Phil. Trans.,' Part II, 1880, "On the Relation between the Diurnal Range of Magnetic Declination and Horizontal Force, as observed at the Royal Observatory, Greenwich, during the years 1841 to 1877, and the Period of Solar Spot Frequency"; 'Monthly Notices, R.A.S.,' December, 1899, vol. 60, No. 2, "On the Relation between Magnetic Disturbance and the Period of Solar Spot Frequency."

[†] 'Phil. Trans.,' Part II, 1880.

Comparison of the Prominences with the Magnetic Disturbance Curves.

If a comparison of the curve representing the number of days of the "great" magnetic disturbance is made with those representing prominence frequency (Plate 4), it will be seen that the former is as unlike the curves representing the prominence frequency about the solar equator as it is like those near the poles; in fact, the polar prominence outbursts, and great magnetic disturbances occur almost simultaneously.

The peculiar form and general similarity of the curves can be best seen from the accompanying illustration (fig. 1). In the figure comparison is made between the epochs of the crossing of the known and unknown lines, the percentage frequency of prominences about the solar poles and Ellis' "great" magnetic disturbances.

Two curves representative of prominence frequency are given, one to indicate the abrupt nature of the curves representing the frequency in a zone near the pole 10 degrees in width (in this case 60°—70° north), and the second to illustrate polar action as a whole; this latter was obtained by making a summation of prominence frequency for the two zones 60°—90° north and south.

The simultaneous occurrence of the maxima suggests that, when the prominence action takes place at the polar regions of the sun, one effect on the earth is that we experience our greatest magnetic disturbances.

Further, according to Mr. Ellis,* "unusual magnetic disturbance is frequent about epochs of sun-spot maximum, and nearly or quite absent about epochs of sun-spot minimum."

We find that not only do these "great" disturbances occur at the same time as the polar prominences, but the spectroscopic observations of sun-spots show that they take place not only "about" the times of spot maximum, as stated by Mr. Ellis, but when the sun-spot curve is *approaching* a maximum and at the dates of the widened line crossings,† when the curve representing the "unknown" lines is on the rise, and crosses the "known" line which is descending. At the other epoch of "crossing," *i.e.*, when the curve showing the "known" lines is on the rise and the "unknown" is falling, there is practically no magnetic disturbance recorded. Attention is again drawn to these crossings, as it is desired to indicate that it is only at those particular times when the sun is increasing his temperature that these disturbances occur.

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are not the same, and in dealing with the curve representing this variation for the whole hemisphere, this fact should be borne in mind.

It may further be noted that the epochs of minima may be practically considered the same for both hemispheres.

* 'Monthly Notices R.A.S.,' vol. 60, p. 148.

† 'Roy. Soc. Proc.,' vol. 67, p. 412.

The second class of phenomena, namely, the magnetic disturbances,
ich are more irregular in occurrence, has been classified by Mr.
lis into five groups, and tabulated by him under five separate sub-
ads. For the present paper, reference will only be made to one of
se classes, namely, that described as "great," this group represent-
the largest disturbances. The curve representing the variation in
mber of these disturbances indicates short intermittent crests, out-
rsts, in fact, with rapid rises to maxima and falls to minima, and
nparatively long intervals of quiescence.

nparison of the Curves representing Prominence Frequency and Variation of Diurnal Magnetic Range.

Mr. Ellis, as already has been pointed out, has indicated the close
emblance between the sun-spot curve and that representing the
riation of the magnetic elements; and it has been shown in the
lier part of this paper, that the curves representing the percentage
quency of prominences near the solar equator, conform in the main
the general sun-spot curve.

There is therefore an apparent connection between phen[illegible]ena oc-
ring in the equatorial regions of the sun (as represented l es of

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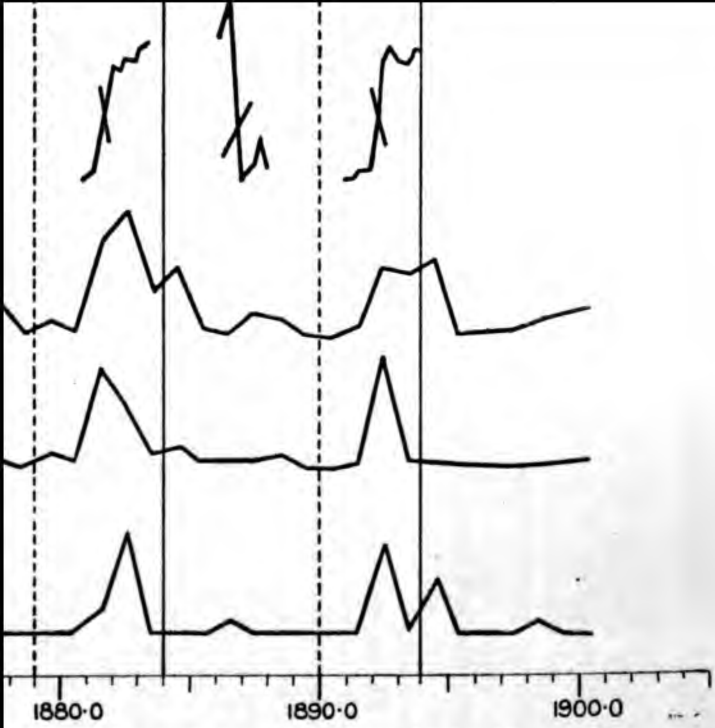


urbance, polar prominences, and crossings of widened lines.
epochs of sun-spot maxima and minima respectively.)

The facts in this paper explain why it is that magnetic storms sometimes take place when there are no spots, or no very large spots, on the surface of the sun. Since the occurrence of magnetic storms is shown to be very closely connected with the solar prominences, there may be prominences and magnetic storms when there are no spots. Prominences may also sometimes be associated with large spots, and as the latter can be seen while the former can not, the resulting magnetic storm is generally attributed to the spots.

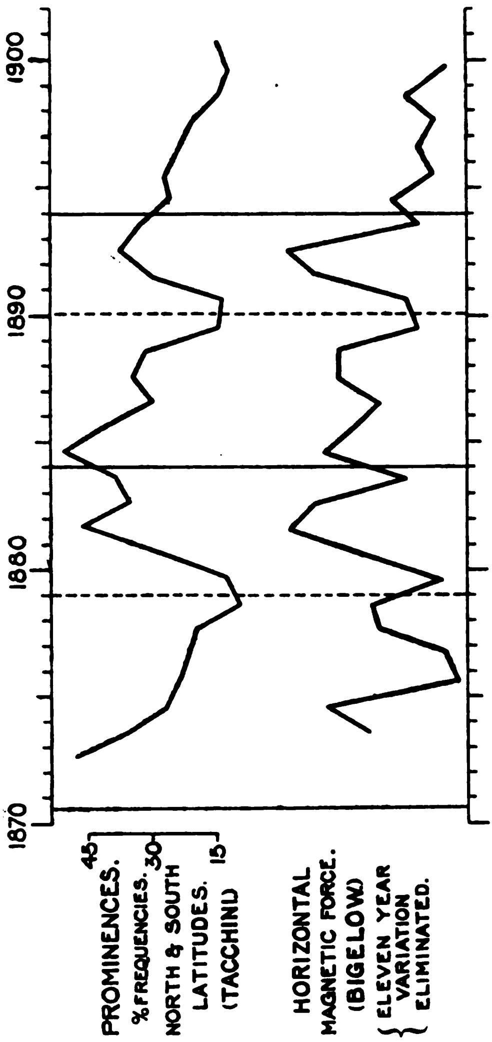


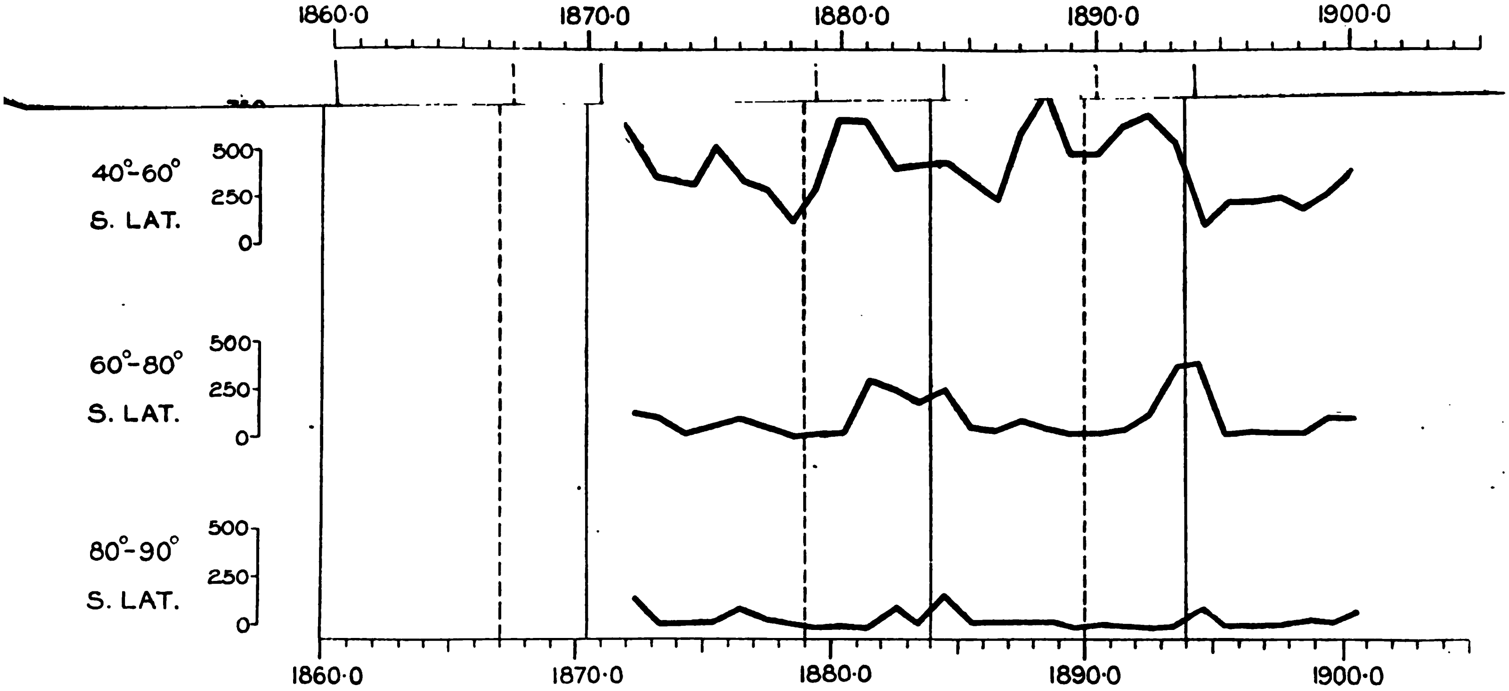
Fig. 2.—Comparison between horizontal magnetic force (Bigelow) and solar prominences (Tacchini). (Continuous and broken vertical lines as in fig. 1.)

rther, the magnitude of magnetic storms appears to vary accord- o the particular position as to latitude of the prominence on the disc. The nearer the poles (either north or south) the prominence s, the greater the magnetic storm, and these are the regions where ots exist.

this paper we have shown that the variations of the general etic phenomena, as given by Ellis, synchronise with the occurrence ominences about the solar equator, while his "great" magnetic rbances occur, in point of time, with the appearance of prominences e polar regions of the sun.

ofessor Bigelow has recently* investigated the variations in the ontal magnetic force, and finds that the curve representing these ges exhibits subsidiary maxima which synchronise with those ded in the curve representing the mean variation of prominences ll latitudes. Thus, to use his own words, "the remarkable hronism between the curves cannot escape recognition, except the year 1894, when an extra minor crest is developed in the ontal force."

e accompanying diagram (fig. 2) gives Professor Bigelow's curve, h represents, as he says, "the series of minor variations which found in the horizontal magnetic force . . . . after the 11-year



Curves showing the percentage frequency of solar prominences for each 20° zone N. and S.
(The continuous and broken vertical lines indicate the epochs of sun-spot maxima and minima respectively.)

ı is of some importance on account of its application to the propa- n of electric waves along the surface of the earth. The case is of a Hertzian oscillator placed outside a perfectly conducting e. Let the radius of the sphere be a, and let the oscillator be at oint C, whose distance OC from O the centre of the sphere is r_1, lirection of the axis of the oscillator being along the line OC. lines of magnetic force are circles, whose centres lie on OC, and planes are perpendicular to OC. If γ denotes the magnetic force y point P whose distance from OC is ρ, $\gamma\rho$ satisfies the differential ion

$$\frac{\partial^2}{\partial\rho^2}(\gamma\rho) - \frac{1}{\rho}\frac{\partial}{\partial\rho}(\gamma\rho) + \frac{\partial^2}{\partial z^2}(\gamma\rho) + \kappa^2\gamma\rho = 0,$$

e z is the distance of the point P from some plane of reference ndicular to OC, and $2\pi/\kappa$ is the wave-length of the oscillations. sforming to polar co-ordinates (r, θ), where $r =$ OP and θ is the COP, this equation becomes

$$\frac{\partial^2}{\partial r^2}(\gamma\rho) + \frac{1-\mu^2}{r^2}\frac{\partial^2}{\partial\mu^2}(\gamma\rho) + \kappa^2\gamma\rho = 0 \quad\ldots\ldots\ldots\ldots\ldots \quad (1),$$

hich $\mu = \cos\theta$. The general solution of this equation, which is cable to the space external to a sphere, is

Now

$$\rho\frac{\partial}{\partial\rho} = (1-\mu^2)\left[r\frac{\partial}{\partial r}-\mu\frac{\partial}{\partial\mu}\right],$$

$$(2n+1)\mathrm{P}_n = \frac{\partial\mathrm{P}_{n+1}}{\partial\mu}-\frac{\partial\mathrm{P}_{n-1}}{\partial\mu},$$

$$(2n+1)\mu\frac{\partial\mathrm{P}_n}{\partial\mu} = n\frac{\partial\mathrm{P}_{n+1}}{\partial\mu}+(n+1)\frac{\partial\mathrm{P}_{n-1}}{\partial\mu};$$

making these substitutions and rearranging the series, it becomes

$$\psi_1 = r_1{}^{-\frac{1}{2}}e^{\pi\iota/4}\sum_1^\infty\left[e^{(n-1)\frac{\pi\iota}{2}}\mathrm{K}_{n-\frac{1}{2}}(\iota\kappa r_1)\left\{r\frac{}{\partial r}r^{-\frac{1}{2}}\mathrm{J}_{n-\frac{1}{2}}(\kappa r)\right.\right.$$

$$\left.-(n-1)r^{-\frac{1}{2}}\mathrm{J}_{n-\frac{1}{2}}(\kappa r)\right\}-e^{(n+1)\frac{\pi\iota}{2}}\mathrm{K}_{n+\frac{3}{2}}(\iota\kappa r_1)\left\{r\frac{\partial}{\partial r}r^{-\frac{1}{2}}\mathrm{J}_{n+\frac{3}{2}}(\kappa r)\right.$$

$$\left.\left.+(n+2)r^{-\frac{1}{2}}\mathrm{J}_{n+\frac{3}{2}}(\kappa r)\right\}\right](1-\mu^2)\frac{\partial\mathrm{P}_n}{\partial\mu},$$

that is

$$\psi_1 = r^{\frac{1}{2}}\sum_1 g_n(r_1)\mathrm{J}_{n+\frac{1}{2}}(\kappa r)(1-\mu^2)\frac{\partial\mathrm{P}_n}{\partial\mu},\ r<r_1 \ldots\ldots\ldots\ldots \quad (2),$$

where

$$g_n(r_1) = -\kappa r_1{}^{-\frac{1}{2}}e^{\pi\iota/4}\left\{e^{(n-1)\frac{\pi\iota}{2}}\mathrm{K}_{n-\frac{1}{2}}(\iota\kappa r_1)+e^{(n+1)\frac{\pi\iota}{2}}\mathrm{K}_{n+\frac{3}{2}}(\iota\kappa r_1)\right\}.$$

If, then, a solution ψ of equation (1) can be found, which is such that ψ becomes infinite as ψ_1 at the point $(r_1, 0)$ and $\frac{\partial\psi}{\partial r}$ vanishes when $r = a$, the real part of $\mathrm{C}\psi e^{\iota\kappa\mathrm{V}t}$ will be γ, the required magnetic force, for then $\frac{\partial}{\partial r}(\gamma\rho)$ will vanish when $r = a$, that is, the electric force tangential to the sphere vanishes. The solution required will be of the form

$$\psi = r^{\frac{1}{2}}\sum_1^\infty g_n(r_1)\left[\mathrm{J}_{n+\frac{1}{2}}(\kappa r)+\mathrm{A}_n\mathrm{K}_{n+\frac{1}{2}}(\iota\kappa r)\right](1-\mu^2)\frac{\partial\mathrm{P}_n}{\partial\mu},$$

where $r_1 > r > a$, and the constants A_n are determined by the condition that $\frac{\partial\psi}{\partial r} = 0$, when $r = a$; hence

$$\psi = r^{\frac{1}{2}}\sum_1^\infty g_n(r_1)\left[\mathrm{J}_{n+\frac{1}{2}}(\kappa r)-\frac{\frac{\partial}{\partial a}\{a^{\frac{1}{2}}\mathrm{J}_{n+\frac{1}{2}}(\kappa a)\}}{\frac{\partial}{\partial a}\{a^{\frac{1}{2}}\mathrm{K}_{n+\frac{1}{2}}(\iota\kappa a)\}}\mathrm{K}_{n+\frac{1}{2}}(\iota\kappa r)\right](1-\mu^2)\frac{\partial\mathrm{P}_n}{\partial\mu},$$

where $r_1>r>a$.

he calculation of the electric forces at a point not on the surface of sphere presents difficulties, but when the wave-length is small pared with the radius of the sphere, the electric force at the surface he sphere can be obtained in a simple form. The electric force at surface of the sphere is normal to the surface, and denoting it by F,

$\frac{F}{\partial t}$ is the real part of $\frac{C}{a^2}\frac{\partial\psi}{\partial\mu}$ when $r = a$. Now

$$\frac{\partial\psi}{\mu_{r=a}} = a^{-\frac{1}{2}}\frac{\partial}{\partial\mu}\sum_1^{\infty} g_n(r_1)\left[J_{n+\frac{1}{2}}(\kappa a) - \frac{K_{n+\frac{1}{2}}(\iota\kappa a)}{\frac{\partial}{\partial a}\{a^{\frac{1}{2}}K_{n+\frac{1}{2}}(\iota\kappa a)\}}\frac{\partial}{\partial a}\{a^{\frac{1}{2}}J_{n+\frac{1}{2}}(\kappa a)\}\right]$$
$$(1-\mu^2)\frac{\partial P_n}{\partial\mu},$$

when the wave-length is small compared with a, this becomes

$$\frac{\partial\psi}{\partial\mu_{r=a}}$$
$$= a^{-\frac{1}{2}}\frac{\partial}{\partial\mu}\sum_1^{\infty} g_n(r_1)\left[J_{n+\frac{1}{2}}(\kappa a) + \frac{a^{-\frac{1}{2}}}{\iota\kappa}\frac{\partial}{\partial a}\{a^{\frac{1}{2}}J^{n+\frac{1}{2}}(\kappa a)\}\right](1-\mu^2)\frac{\partial P_n}{\partial\mu},$$

is, writing $\psi_1 = f(r)$,

and therefore

$$F = -\frac{CV}{\kappa a^2}\frac{\partial}{\partial\mu}\left[(1-\mu^2)\frac{a^2}{R_0}\frac{\partial}{\partial R_0}\frac{\sin\kappa(R_0-Vt)}{R_0}\right.$$

$$\left. + \frac{1}{\kappa}\frac{\partial}{\partial a}\frac{a^2}{R_0}\frac{\partial}{\partial R_0}\frac{\cos\kappa(R_0-Vt)}{R_0}\right\}\Bigg] \quad\ldots\ldots\ldots\ldots\ldots(3).$$

At points on the surface of the sphere for which R_0 is great compared with the wave-length, this becomes, retaining only the most important terms,

$$F = -CV\frac{\kappa a r_1}{R_0^3}\left(1-\frac{a-r_1\mu}{R_0}\right)(1-\mu^2)\sin\kappa(R_0-Vt),$$

or

$$F = \left(1-\frac{a-r_1\mu}{R_0}\right)F_1 = (1-\cos\chi)\,F_1 \quad\ldots\ldots\ldots\ldots\ldots\ldots(4),$$

where χ is the angle subtended by OC, and F_1 is the electric force along the normal, which would be due to the oscillator if the sphere were absent. From this it follows that the ratio F/F_1 gradually diminishes as μ decreases, until μ approaches the value -1, when it becomes comparable with the terms which have been neglected. Hence, when electric waves are incident on a perfectly conducting sphere, there is no true shadow near the surface when the wave-length is small compared with the radius of the sphere. It can, therefore, be inferred that, when electric waves are incident on a perfectly conducting body whose surface is convex, and has its radii of curvature everywhere great compared with the wave-length, there is no true shadow near to the surface. It is known that, when electric waves of small wave-length are incident on a perfectly conducting wedge, the disturbance does not sensibly creep round the corner, but shoots out so that there is a shadow which coincides the more closely with the geometrical shadow as the wave-length diminishes.* It therefore appears that the condition for the formation of a distinct shadow near the surface of a perfectly conducting body, whose surface is convex, when electric waves are incident on it, is that there should be a line on that part of the surface inside the geometrical shadow along which the radius of curvature of the surface in the plane of incidence of the waves is small compared with the wave-length.

3. The electric force normal to the surface of a sphere, which is a fairly good conductor, may be obtained by an analysis similar to that given above. The result is

$$\frac{1}{a^2}\frac{\partial\psi}{\partial\mu_{r=a}} = \frac{1}{a^2}\frac{1-\iota\epsilon}{1+\epsilon^2}\frac{\partial}{\partial\mu}\left[f(a)+\frac{1}{\iota\kappa}\frac{\partial}{\partial a}f(a)\right],$$

---

* Sommerfeld, 'Math. Annalen,' vol. 47, 1896; or Macdonald, 'Electric Waves,' 1902, p. 187.

ere $\epsilon = \sigma\kappa/4\pi V$, and $\gamma_1\rho$ is the real part of $f(r)$, and σ is the specific stance of the material of the sphere, the other symbols having the e meaning as before; it being assumed that ϵ, which for ordinary allic conductors is of the order 10^{-7} when the wave-length is about cm., is small. If F′ now denotes the electric force normal to the face, F′ differs in phase from F (the electric force normal to the face when the sphere is a perfect conductor) by a small amount, the ratio of the amplitude of F′ to F is $1 - \frac{1}{2}\epsilon^2$. The effect of erfect conduction is therefore to diminish the electric force normal he surface, but only by an inappreciable amount when the obstacle s good a conductor as an ordinary metal; for sea water, taking 10^{10}, the correction is less than one part in a thousand.*

. The effect of a rigid spherical obstacle on the waves of sound t out from a source, when the wave-length is small compared with radius of the sphere, can be obtained by an analysis which s ost identical with that given above. The result is that at any nt on the sphere at a distance from the source great compared with wave-length,

$$\phi = \phi_1\left(1 - \frac{a - r_1\mu}{R_0}\right) = \phi_1(1 - \cos\chi),$$

ere ϕ_1 is the velocity potential at the point due to the source, and convenient for purposes of comparison to substitute for F_1 in terms of another quantity; let F_2 denote the electric force due to the oscillator at a point in its equatorial plane at a distance $a\theta$ from the oscillator, which is the same as the arcual distance of the receiver; then

$$\frac{F}{F_2} = \frac{\theta \cos^2 \frac{1}{2}\theta (1 - \sin \frac{1}{2}\theta)}{2 \sin \frac{1}{2}\theta} = k,$$

where $\bar{F}$ is the amplitude of F, and $\bar{F}_2$ the amplitude of F_2; the ratio of the intensities in the two cases is k^2. The following table shows the manner of variation of the amplitudes and the intensity (k^2) near the sphere as θ increases from 20° to 120°:—

θ	$1-\sin\frac{\theta}{2}$	k	k^2
20°	0·82635	0·80551	0·64885
25°	0·78356	0·73567	0·54121
30°	0·74118	0·69949	0·48929
35°	0·69929	0·64546	0·41661
40°	0·65797	0·59297	0·35162
45°	0·61731	0·54070	0·29235
50°	0·57738	0·48964	0·23975
55°	0·53825	0·44019	0·19377
60°	0·5	0·39269	0·15421
65°	0·46270	0·34745	0·12072
70°	0·42642	0·30473	0·09286
75°	0·39127	0·26477	0·07010
80°	0·35721	0·22766	0·05183
85°	0·32440	0·19343	0·03741
90°	0·29289	0·16266	0·02645
95°	0·26272	0·13483	0·01818
100°	0·23395	0·11011	0·01212
105°	0·20664	0·08844	0·00782
110°	0·18084	0·06972	0·00486
115°	0·15660	0·05379	0·00289
120°	0·13397	0·04050	0·00164

For example, when $\theta = \frac{1}{4}\pi$, that is for the case of the earth at a distance of rather more than 3000 miles, the amplitude of the electric force acting on the receiver is more than half the amplitude of the electric force that would be directly due to the oscillator at that distance, and the intensity nearly three-tenths. These results will apply when the two places are separated by good conducting material such as sea water, the effect of the imperfect conduction of such substances being by § 3 negligible. They explain why wireless telegraphy is more effective over the sea or wet soil than over dry soil; from § 3 it follows that a badly-conducting obstacle diminishes the effect. It is also to be expected from § 2 that the influence of a lge of some sharpness between the places is to create a distinct adow, to such an extent that the effect would be inappreciable; the me result would be produced by an intervening headland; this rees with the experience of Captain Jackson.*

Studies in the Morphology of Spore-producing Members.—No. V. General Comparisons, and Conclusion." By F. O. Bower, Sc.D., F.R.S., Regius Professor of Botany in the University of Glasgow. Received January 30,—Read February 12, 1903.

(Abstract.)

This concluding Memoir contains a general discussion of the results quired in the four previous parts of this series, and of their bearing a theory of sterilisation in the sporophyte. The attempt is made build up the comparative morphology of the sporophyte from low, by the study of its simpler types; the higher and more ecialised types are left out of account, except for occasional comrison. It is assumed for the purposes of the discussion that alter-

but varies both in occurrence and origin; that even the individuality of the sporangium is not always maintained. All that remains then as the fundamental conception of the sporangium in vascular plants is the spore-mother-cell, or cells, and the tissue which covers them in, for such cells are always produced internally. The definition of the sporangium may then be given thus: "*Wherever we find in vascular plants a single spore-mother-cell, or connected group of them, or their products, this with its protective tissues constitutes the essential of an individual sporangium.*" From the point of view of a theory of sterilisation such sporangia may, at least in the simplest cases, be regarded as islands of fertile tissue which have retained their spore-producing character, while the surrounding tissues have been diverted to other uses. It will be seen later how far this view will have to be modified in the more complex cases.

In a second section of the Memoir the variations in number of sporangia in vascular plants are discussed; the methods of variation may be tabulated as follows, under the heads of progressive increase and decrease:—

I.—*Increase in Number of Sporangia.*

(*a.*) By septation, with or without rounding off of the individual sporangia.
(*b.*) By formation of new sporangia, or of new spore-bearing organs, which may be in addition to, or interpolated between those typically present.
(*c.*) By continued apical, or intercalary growth of the parts bearing the sporangia.
(*d.*) By branching of the parts bearing the sporangia.
(*e.*) Indirectly, by branchings in the non-sporangial region resulting in an increased number of sporangial shoots; this is closely related to (*c*) and (*d*).

II.—*Decrease in Number of Sporangia.*

(*f.*) By fusion of sporangia originally separate.
(*g.*) By abortion, partial or complete, of sporangia.
(*h.*) By reduction or arrest of apical or intercalary growth in parts bearing sporangia.
(*i.*) By fusion of parts which bear the sporangia or arrest of their branchings.
(*j.*) Indirectly, by suppression of branchings in the non-sporangial region, resulting in decreased number of sporangial shoots; this is closely related to (*h*) and (*i*).

We are justified in assuming that (subject to the possibility of other factors having been operative of which we are yet unaware) e condition of any polysporangiate sporophyte as we see it is e resultant of modifications such as these, operative during its scent.

The problem will, therefore, be in each case to assign its proper ace in the history to any or each of these factors.

It is pointed out that in homosporous types, which are certainly e more primitive, the larger the number of spores the better the ance of survival, and hence, other things being equal, increasing mbers of spores and of sporangia may be anticipated; but in the terosporous types reduction in number both of spores and of orangia is frequent. The former will accordingly illustrate more thfully than the heterosporous forms the story of the increase of mplexity of spore-producing parts. The general method put in actice here is to regard homosporous forms as in the upgrade of eir evolution, as regards their spore-producing organs, unless there clear evidence to the contrary. The *onus probandi* lies rather th those who assume reduction to have taken place in them.

A summary of evidence of variation in number of sporangia by any these methods is then given for the Lycopodineæ, Psilotaceæ, henophylleæ, Ophioglossaceæ, Equisetineæ, and Filicineæ; followed each case by a theoretical discussion of the bearing of that the decentralisation of the fertile tissue in the primitive Pteridophytes as in the Bryophytes, and result in the formation of a central sterile tract, with an archesporium at its periphery; that such an archesporium, instead of remaining a concrete layer as it is in the larger Musci, became discrete in the Lycopods; that the fertile cell-groups formed the centres of projecting sporangia, and that they were associated regularly with outgrowths, perhaps of correlative vegetative origin, which are the sporophylls.

Whether or not this hypothesis of the origin of a Lycopod strobilus approaches the actual truth, comparison points out the genus *Lycopodium* as a primitive one, characterised by more definite numerical and topographical relation of the sporangia to the sporophylls than in any other type of Pteridophyta.

Then follows, as a consequence of comparison, the enunciation of a theory of the sporangiophore, a word which is here used in an extended sense to include not only the spore-producing organs of Psilotaceæ, Sphenophylleæ, Ophioglossaceæ, Equisetaceæ, but also the sori of Ferns. The view is upheld that all these are simply placental growths, and not the result of "metamorphosis" of any parts or appendages of prior existence; that the vascular supply, which is not always present, is not an essential feature; that they are seated at points where in the ancestry spore-production has been proceeding on an advancing scale; hence they do not occupy any fixed and definite position. It seems probable that at least a plurality of sporangia existed on primitive sporangiophores, and that where only one exists that condition has been the result of reduction.

The above theories are then applied to the several types of Pteridophyta. The Lycopods, Psilotaceæ, Sphenophylleæ, and Ophioglossaceæ may be arranged as illustrating the increased complexity of the spore-producing parts, and of the subtending sporophylls; the factors of the advance from the simple sporangium to the more complex sporangiophore are, septation, upgrowth of the placenta with vascular supply into it, and branching, with apical growth also in the Ophioglossaceæ. But even in the most complex forms the sporangiophore may be regarded as a placental growth, and not the result of transformation of any other member.

In the case of *Helminthostachys* the marginal sporangiophores are regarded as amplifications from the sunken sporangia of the *Ophioglossum* type; in *Equisetum* they are regarded as being directly seated on the axis, and having originated there by a similar progression: they would thus be non-foliar. It is pointed out that though a foliar theory would be possible for *Equisetum* itself, it is not applicable to the facts known for the fossil Calamarieæ, which are so naturally related to it. Thus the strobilus of the Equisetineæ is of a rather different type from that of the Lycopods, Psilotaceæ, or even the hioglossaceæ, in all of which there is a constant relation of the re-producing parts to the leaves; in the Equisetineæ no such connt relation exists; the leaves and sporangiophores may be in juxtaition, as in *Calamostachys*, without exactly matching numerically; the sporangiophores may occur in larger numbers and in several ıks, between successive leaf-sheaths, as in *Phyllotheca* and *Bornia*; without any leaves at all, as in *Equisetum*. Thus, on a nonyllome theory the latter may be held to be only an extreme case of at is seen in certain fossils.

The Ferns, notwithstanding their apparent divergence of character m other Pteridophytes, may also be regarded as strobiloid forms, h greatly enlarged leaves; the primitive sori of the Simplices emble the sporangiophores of other Pteridophytes; the more comcated soral conditions of the Gradatæ and Mixtæ were probably ivative from these, the chief difference being due to the interpolation new sporangia, an innovation which is in accordance with biological bability, as well as with the palæontological record.

The effect of the results thus obtained on the systematic grouping the Pteridophytes is then discussed; it is pointed out that the copods, Psilotaceæ, Sphenophylleæ, Ophioglossaceæ, and Filices strate lines of elaboration of a radial strobiloid type, with increasthe gametophyte of the Psilotaceæ and Sphenophylleæ are not available in this comparison.

It is not obvious what the meaning is of this parallelism between leaf-size and characters of the sexual organs; a further difficulty in its interpretation lies in the fact that for the Equiseta the parallelism does not hold; there "Filicinean" characters of the gametophyte accompany entirely non-Filicinean characters of the sporophyte, the latter showing nearer analogy to the Lycopods than to the Ferns. Such cross characters are difficult to harmonise with any phylogenetic theory; on account of them, the Equisetineæ are placed in an isolated position, and in the same way, though with less pressing grounds, a separate position should be accorded to those types which lie between the extremes of Lycopods and Ferns, in proportion as the characters are more or less pronounced.

On this basis the Isoetaceæ would probably best take their place as a sub-series of the Lycopodiales, Ligulatæ; the Psilotaceæ and Sphenophylleæ would constitute a series of Sphenophyllales, separate from, but related to, the Lycopodiales. The Ophioglossaceæ would form an independent series of Ophioglossales, more aloof than the latter from the Lycopodiales, but not included in the Filicales. The actual connection of these series by descent must remain open; it is quite possible that some or all of them may have originated along distinct lines from a general primitive group, which may be provisionally designated the Protopteridophyta; these were probably small-leaved strobiloid forms, with radial type of construction, and with the sporangia disposed on some simple plan. The grouping arrived at in these Memoirs may be tabulated as follows:—

PTERIDOPHYTA.

- I. LYCOPODIALES.
  - (*a*) Eligulatæ.
    - Lycopodiaceæ.
  - (*b*) Ligulatæ.
    - Selaginellaceæ.
    - Lepidodendraceæ
    - Sigillariaceæ.
    - Isoetaceæ.
- II. SPENOPHYLLALES.
  - Psilotaceæ.
  - Sphenophyllaceæ.
- III. OPHIOGLOSSALES.
  - Ophioglossaceæ.

- IV. FILICALES.
  - (*a*) Simplices.
    - Marattiaceæ.
    - Osmundaceæ.
    - Schizaeaceæ.
    - Gleicheniaceæ.
    - Matonineæ.
  - (*b*) Gradatæ.
    - Loxsomaceæ.
    - Hymenophyllaceæ.
    - Cyatheaceæ.
    - Dicksonieæ.
    - Dennstaedtiinæ.
    - Hydropterideæ (?).
  - (*c*) Mixtæ.
    - Davallieæ.
    - Lindsayeæ.
    - Pterideæ, and other Polypodiaceæ.
- V. EQUISETALES.
    - Equisetaceæ.

but with regard to isolated nerves contradictory statements have been made, and it was to ascertain if possible the reason of this discrepancy that the present research was undertaken.

Methods.

The following method was employed in all the experiments here quoted. The animal was killed by decapitation, and the body left undisturbed for 30—45 minutes. The nerves were then dissected out, placed in a 1·05 per cent. NaCl solution at about 30° C., and kept at this temperature for about half-an-hour or more. They were then allowed to cool to room temperature (17—19° C.), and it was found that, as a rule, the negative variation of nerves so treated was of the order of 1 millivolt (*vide infra*). This, of course, is not an absolute value of the true P.D. between active and inactive parts of nerve, but only a fraction of it, depending upon the amount of internal derivation in a nerve trunk by indifferent conducting tissue.

From about 2 to 6 hours *post-mortem* this value remains at a fairly constant amount, for instance, in Experiment B^d, the sciatic of the rabbit was used 5 h. 30 m. *post-mortem*, and gave a negative variation of 0·00083 volt; in Experiment A^k, 2 h. 40 m. *post-mortem* the value was 0·00076 volt, and these are typical instances. The table on p. 277 gives the result of twenty-two experiments, in which the values were taken for heat determination, which illustrates this.

Certain points may be here noted. The practice of placing nerves in salt solution for some time before use has been employed by Waller,[*] Gotch,[†] and Boycott,[‡] in the case of the frog.[§]

The effect of changes in the composition of the salt solution is the subject of another research which I hope to publish at a future time; however, I may here state that small differences in the concentration of the solution—*e.g.*, ± 0·1 per cent. NaCl—make no apparent difference in the condition of the nerve, and the same is true in the main of *small* differences in the reaction, and of small differences of temperature.

Waller[||] has pointed out that the presence of lactose in the solution is of advantage, and taking a greater value of the negative variation for a sign of greater irritability, the same appears to be true in mammalian nerve for maltose and glucose, though I make the statement at present with some reserve.

For instance:—Experiments B^{tc} and B^{td}.

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[*] Waller, 'Brain,' vol. 73, 1896, p. 43, *et seq.*

[†] Gotch, 'J. Physiol.,' vol. 28, p. 32.

[‡] Boycott, *loc. cit.*

[§] See also Gotch and Horsley, *loc. cit.* Macdonald and Reid, *loc. cit.*

[||] Waller, *loc. cit.* (Lectures), p. 73.

Young Rabbit.

Nerves kept for 3^h circa *post-mortem.*

Experiment No.	Temp. of nerve.	Volt. of neg. var. at excit. 100.	Volt. of neg. var. at excit. 30.	Notes.
B^{1c}	30° C.	0·00033	0·00023	1·05 p. c. NaCl + 0·5 p. c. maltose. R. sciatic.
B^{1d}	30° C.	0·00023	0·00016	1·05 p. c. NaCl only. L. sciatic.

he two nerves of opposite sides are here compared, and the comrison is in favour of the R. sciatic which had been treated with altose. No account is here possible of the precautions used to clude fallacy, but many further experiments support the one quoted, d it is at least very probable that the maltose is the active variant.

The method employed for determining the negative variation and e action of anæsthetics was in all cases that of Waller. An addional larger box was used, with a false bottom of wood, on which the

The question of temperature will be considered later. Assuming for the present purpose that the conditions of experiment were equally favourable throughout, and taking the sciatic nerve as a standard, different animals gave the following values:—

	Millivolts.		
	Maximum observed.	Mean.	
Rabbit	1·1	0·69	Mean of 11 experiments.
Kitten	0·66	0·50	„ 5 „
Pigeon	1·05	0·42	„ 5 „
Guinea-pig	1·2	0·89	„ 2 „

All these are very much less than the frog, which gives 2 millivolts or more.

Different nerves in the same animal often show individual inequalities, but as a rule the larger nerves give a smaller negative variation than those of less diameter. The sciatic gives commonly the least, but is the most resistant to adverse influences. The median and ulnar nerves are more delicate, but give larger variation under favourable circumstances, *e.g.*, the median nerves of the pigeon gave a mean value of 0·54 millivolt (five experiments) as against 0·42 for the sciatic. The greatest value I have yet measured was in a branch of the anterior crural of the rabbit, which on the right side gave 2·5 millivolts and on the left 2·3.

Boruttau* found the vagus in the rabbit to give a larger negative variation than the sciatic, and obtained only very small responses from the nerves of hens, ducks, or pigeons.

The larger number of fibres not in contact with the longitudinal electrode would appear to act as a deriving circuit of less resistance in the larger nerves, and so less current passes through the galvanometer, and the greater amount of connective tissue in the sciatic would have the same effect. It is possible that there are other causes in addition to these; there is at present no evidence for or against such a possibility.

Similar reasons probably also explain why a stronger stimulus is necessary for the warm-blooded nerves than for the frog. The difference is, however, not very great. The minimal effective excitation I have so far observed is 6 units of the "Berne" coil, 500 units is commonly a maximum, 1000 nearly always so. The smaller electrical esistance of the mammalian nerve between the exciting electrodes ust be borne in mind in these comparisons.

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* Boruttau (*a*), *loc. cit.*

Gotch* has recently stated that in determining the sub-maximal esponse of frog's nerve, the excitation of a smaller number of fibres is far more potent cause than the varying response of each fibre, and it eems very probable that the higher threshold and wide range of xcitation in mammalian nerves is due to the failure of the exciting urrent to reach the more distant fibres, protected as they are by interening fibres and connective tissue, and not to any essential difference n the nerves.

The negative variation commonly persists without great alteration nder ordinary conditions for at least 4—8 hours *post-mortem.* The ongest time I have seen was in Experiment C^{df} (internal popliteal of he hedgehog, 28 hours *post-mortem*); the right and left median nerves he kitten in Experiments C^{cg} and C^{cf} gave a small and rapidly iminishing response 19 hours *post-mortem.*

The earlier observers (Valentin, Fredericq, Hermann) have stated hat they have found the negative variation to persist for days, and to ast longer than in frog's nerve. I am unable to confirm this; even n the hedgehog the nerves are much more short-lived than in the rog under similar conditions, and the phenomena referred to were robably of a different nature to those examined here viz. electro-

Action of Anæsthetics.

The following is the summary of the observations made on this subject:—

Chloroform.

Experiment and plate No.	Negat. var. before.	During $CHCl_3$.	After.	Notes.
204	$\frac{1}{1000}$ volt. 0·48	0·42 to 0 3 min.	0 to 0·42	Kitten. Sciatic. Temp. of nerve chamber, 32°. Complete abolition and recovery.
207	0·38	0·38 to 0 5 min.	0 to 0·22	Kitten. Sciatic. Temp. = 30° C. After $CHCl_3$ a positive variation appeared, changing again to a negative. Fair recovery.
A° 408	0·95	0·70 to 0·40 3 min.	0·21 to 0·13	Rabbit. L. sciatic. 6 h. *post-mortem.* Temp. = 20°. Gradual progressive diminution and no recovery. *FIG.* 3.
C^{rr} 424	0·46	0·46 to 0·15 4 min.	0·18 to 0·36	Kitten. L. median. Temp. = 32°. $CHCl_3$ dilute at first, stronger after first minute.
B^{1} 417	0·47	0·6 to 0·1 5 min.	0·21 to 0·40	Pigeon. R. median. 3 h. *post-mortem.* Temp. 37°. Recovery.

Ether.

		During ether.		
201	0·40	0·25 to 0 3 min.	0 to 0·28	Kitten. R. sciatic. Temp. 34°. 37 m. *post-mortem.* Imperfect recovery.
203	0·66	0·25 to 0 3 min.	0 to 0·46	Kitten. L. sciatic. 4 h. *post-mortem.* Temp. = 28°. Recovery.
B^{b} 413	0·54	0·22 to 0 3 min.	0 to 0·54	Rabbit. Ext. popliteal. 2 h. 30 m. *post-mortem.* Temp. = 30°. Recovery. *FIG.* 2.
B^{b} 416	0·83	— to 0 2 min.	0 to 0·51	Pigeon. 2 h. 30 m. *post-mortem.* Temp. = 39° C. L. sciatic. Recovery.

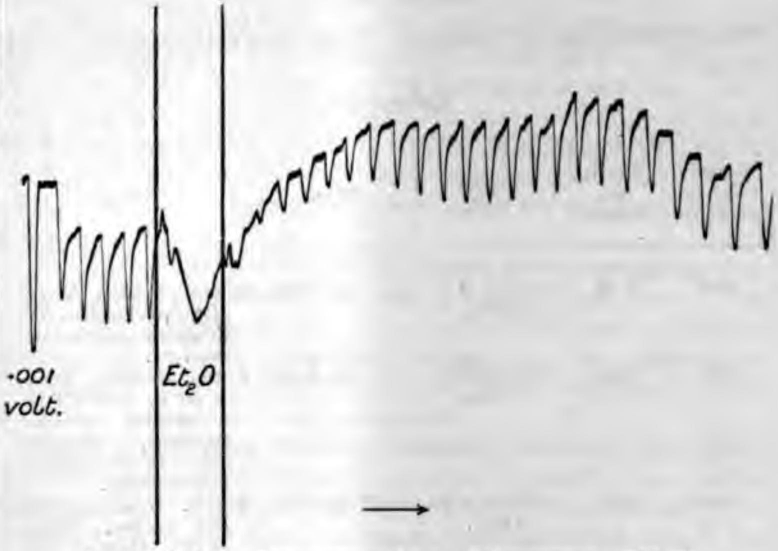

FIG. 2.—Rabbit. Same nerve as fig. 1. Ether vapour. Exp. B^b.

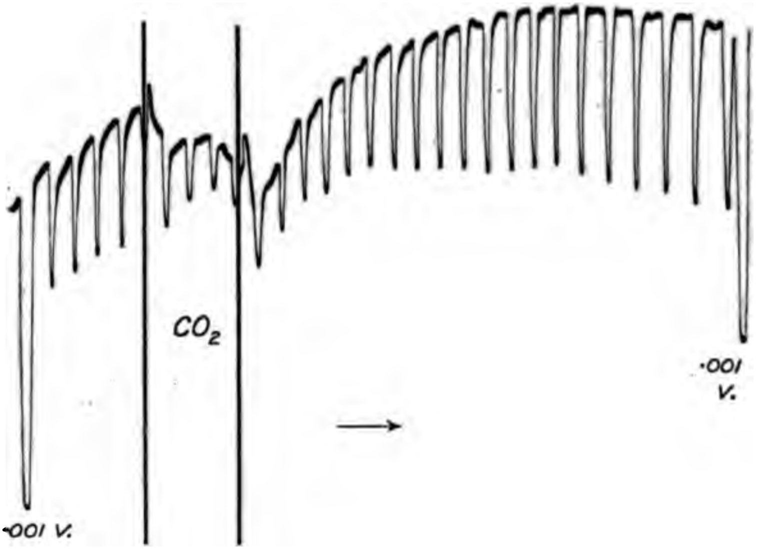

FIG. 4.—Rabbit. R. sciatic. CO_2. Exp. A¹.

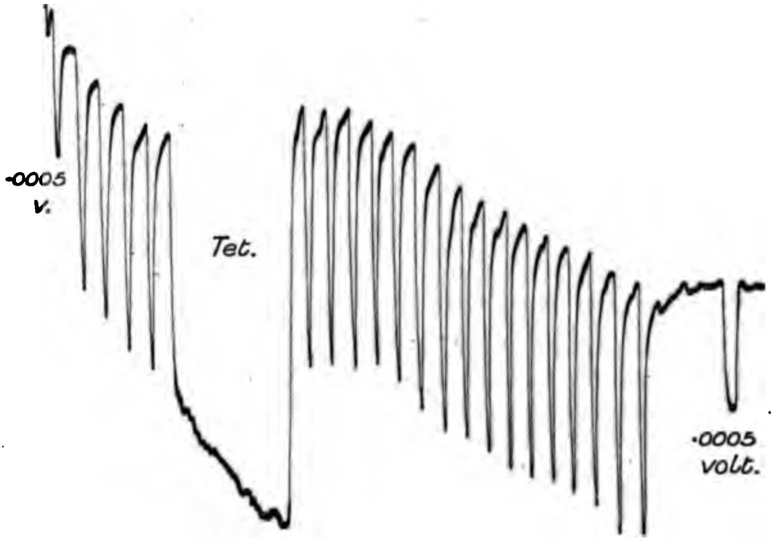

FIG. 5.—Rabbit. R. sciatic. Tetanisation for 5 m. Exp. A¹.

CO_2.

Experiment and plate No.	Neg. var. before in 0·001 volt.	Neg. var. during CO_2.	Neg. var. after.	Notes.
202	0·59	0·1 3 min.	0·3, 0·66, 0·53	Kitten. L. sciatic. 3 h. *post-mortem.* Temp. = 30°. Primary diminution and secondary augmentation.
A^i 406	0·39	0·29 to 0·15 4 min.	0·16 to 0·61	Rabbit. R. sciatic. 3 h. 45 m. *post-mortem.* Temp. 22° C. As 202. Marked secondary augmentation. *FIG.* 4.
A^j 405	0·70	0·56 to 0·3 5 min.	0·35 to 0·97	Rabbit. L. sciatic. 1 h. 10 m. *post-mortem.* As 406.
B^m 418	0·32	0·3, 0·42, 0·23 6 min.	0·64 to 0·33	Pigeon. L. sciatic. 3 h. *post-mortem.* Temp. = 37° C.

Tetanisation.

observed in the bird (Experiment B^1) and not in the mammal. In the case of CO_2 this increase is seen in all the warm-blooded nerves; primary and secondary augmentation are shown in Exp. B^m from the pigeon, and the latter, in fig. 4, from the rabbit.

I do not here enter upon any discussion of the actual mechanism of this increase, it may be due to either of two causes—increase of E.M.F. or increased duration of electromotive change. The latter explanation has been suggested by Gotch as being the true one.

The effect of tetanisation (fig. 5) has not been marked. Three experiments proved negative, and two gave a slight increase, so that this question is still undecided.

It appears, therefore, certain that neither in the voltage of the negative variation, in the strength of excitation, or in the action of anæsthetics is there any marked difference between the warm-blooded and amphibian nerves, and that all the facts ascertained for the latter under these heads can be applied *en bloc* to the former.

Temperature of Extinction by Heat.

Three series of experiments on frogs, mammals, and birds were undertaken to ascertain the precise point at which the negative variation was abolished by heat.

Method.—The nerve chamber was kept at a constant temperature throughout, *e.g.*, 30° C. The nerve itself was placed on the electrodes, and when it had reached the temperature of the chamber, the value of 0·001 volt was determined on the galvanometer scale, and then the values of the first six negative variations. The nerve was then removed, placed in 1·05 per cent. NaCl solution (containing Ca salts, &c.) at the desired high temperature (*e.g.*, 49° C.), left for exactly 5 minutes, placed in cool (18° C.) salt solution for 7 minutes. A fresh transverse section was made, the nerve was replaced on the electrodes, and the value of 0·001 volt and the second set of six negative variations determined.

This method fulfils several *desiderata.*

(1.) It is possible to keep the beaker of hot saline solution at any given temperature with an error of less than ± 0·1° C.

A standardised thermometer was placed in the bath close to the nerve, and with 5 minutes immersion all parts of the nerve reach the temperature of the solution.

(2.) Any alteration in the resistance of the nerve is readily detected by means of the standard deflection with 0·001 volt, and as both readings are taken at the same temperature, this alteration must be a permanent one, and not the temporary alteration always seen when a nerve is heated or cooled.

(3.) Using a concentration of salt solution* that had been found to rk equally well with all classes of nerve, and carefully preserving ilar conditions of experiment, the results in the different series e strictly comparable not only with each other, but also with Hallirton's* researches on the heat-coagulation of the nerve proteids.

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* No carbohydrate was added to the solution in any of the heat experiments. The solution was neutral.

The excitation was a maximal one throughout.

Series I.

A. With Laboratory Frogs.
B. With freshly-caught vigorous Frogs.
Sciatic nerve. Excitation 30, except in A^e.

xperiment No.	Hours *post-mortem.*	Temp. of nerve chamber.	Temp. of hot bath.	Neg. var. initial $= a$. 0·001 v.	Neg. var. final $= b$.	Irritability quotient $= \frac{b}{a}$.	Notes.
	h. m.						
A^c	1 0	room	43°·5	1·0	0	0	A.
A^f	2 7	,,	40·7	0·8	0	0	
A^{ha}	5 0	,,	40·0	1·2	0	0	
A^{hb}	5 30	,,	39·0	1·8	0·56	0·31	
A^g	2 30	,,	38·5	1·7	1·7	1·0	

Series III.

Pigeon. Experiments B^n and B^g Sciatic, all the rest Median.

Experiment No.	Hours *post-mortem.*	Temp. of nerve chamber.	Temp. of hot bath.	Neg. var. initial = *a*. 0·001 v.	Neg. var. final = *b*.	Irritability quotient = $\frac{b}{a}$.	Notes.
	h. m.						
C^{fb}	3 15	28°	53°·6	0·30	0	0	Cold bath accidentally omi ted. The nerve was muc contracted longitudinall after heating.
C^{fc}	4 0	30	53·0	0·75	0	0	Nerve contracted. No "n gative variation" aft heating, but large pos tive current escape ol served, not abolished b crushing.
B^n	4 15	38	52·5	0·25	0·12	0·48 }	Neg. var. rapidly diminishing.
B^k	4 30	38	52·0	0·56	0·17	0·30 }	
B^j	3 0	37	50·0	0·61	0·18	0·30	
B^g	2 0	30	45·8	0·30	1·05	3·5	

The experiments can be summarised thus :—

	Normal temp. of animal.	Temp. of incr. neg. var.	Temp of dim. neg. var.	Temp. of abolished neg. var.
Frog	—	39—40° C.	39—41°	40—42°
Rabbit.....	37—41°[*]	42·8°	44·3—47·7°	48—49°
Pigeon.....	40—12·5[†]	45·8°	50°	52—53°

It is seen that the effect of heat occurs in three stages. In the first, at a temperature of 1—2° above that of the animal, the negative variation is increased. In the second there is diminution, recovered from at the lower temperatures (4° over normal) if the nerve is cooled longer than the standard time, not recovered from at the higher (6—7° over normal), and finally the negative variation is permanently abolished, 8° over normal in the rabbit, 10° in the pigeon.

While the mammalian and avian nerves show quite small individual

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[*] Pembrey (Schäfer's 'Text-book,' vol. 1, p. 790). The higher limit for the rabbit is from unpublished observations of Dr. Pembrey, which he has very kindly furnished me for this paper.

[†] Corin and Van Beneden, 'Arch. de Biol.,' Gand., 1887, vol. 7, p. 265.

fferences in different animals as regards their reaction to heat, the
og's nerve varies a little according to the condition of the animal,
d so the observations have been arranged in two divisions. Here
e also notices that the "injury range" is very much smaller than in
e warm-blooded nerve, 2° at most separating a temperature that
s no ill effect for one that finally kills the nerve, as against 5—6° in
e mammal and bird.

Observations.

A summary of the previous work on the effect of temperature on
rves is to be found in Howell's[*] paper, and in that of Boycott
c. cit.).

Howell, from his own researches, gives 41—44° as the temperature
which conductivity is abolished in frog's nerve, the other authors
ve 45—50°. The difference appears to be due to the methods em-
oyed. Hitherto, there has been some difficulty in ensuring that all
rts of the nerve shall have the same temperature, and this tem-
rature has in most cases been ascertained indirectly, further, the
ne during which the temperature is kept up and the conditions of
oisture, &c., greatly influence the results.[†]

Another explanation is possible. The majority of observers[‡] have
amined the conductivity of nerve as opposed to the excitability, and

rigor at 45—50°C,* and the nerves die at 48—49°. The proteids of the cat's brain coagulate at 47° C.† No data for the bird are available,‡ the nerves die at 53°.

In table form.

	Frog.	Mammal.	Bird.
Muscle proteid (Halliburton and von Fürth)	40°	47°	—
Muscle rigor (Vincent and Lewis).........	38—40°	45—48°	—
Nerve proteid (Halliburton)..............	—	47°	—
Nerve electrotonic currents (Waller)	40°	—	—
Nerve (present experiments)..............	40—42°	48—49°	53°

It is reasonable to conclude from these figures that the extinction of the irritability of the nerve is due to the coagulation of the proteids which enter into its composition, and I venture to forecast, that when the proteids of the frog's nervous system are examined one will be found to coagulate at 40°, and that the two proteids coagulating at 40° and 47° are absent from the nerves of the bird. It is possible, therefore, to make a nearer approach to the analysis of actually living nerve substance than has been practicable hitherto.

Temperature of Extinction by Cold.

Method.—Alongside the nerve in the nerve-chamber, was placed a junction (A) of konstantan and iron wire, and the nerve was arranged so as to touch this. The junctions konstantan copper (B) and iron-copper (C) were placed in glass tubes and immersed in water at room-temperature, the two copper terminals led to a key-board, with connections to a sensitive Kelvin-type galvanometer of low resistance (16 ohms), and a compensating circuit arranged as shown. The wire rheochord marked 1 ohm was of the ordinary du Bois-Reymond type, and with the voltage and added resistance as marked 1° difference between the junction A, and B C, was represented by about thirty-five scale divisions. The compensating current was furnished from an accumulator of large capacity. It was found after careful tests that no perceptible alteration (within 0·05°) of the nperature of the fixed junction took place, if it did, a correction ıld be readily applied to the figures obtained. To guard against rrent escape from one circuit to another all the wires leading to the rve chamber were placed within rubber tubing, and the konstantann junction (A) was coated with rubber "tyre-repairing" solution, ich on drying left a thin and even coat of rubber on the surface, sulating it from any nerve currents and from any possible mutual tion from or to the nerve. It was found by experiment that no such tion occurred.

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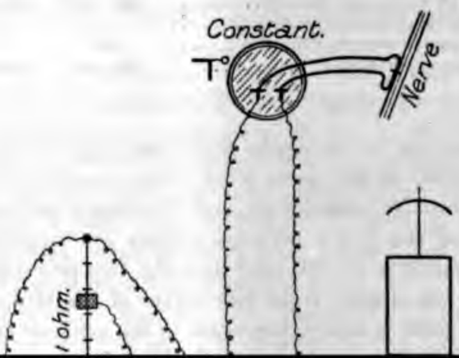
* Vincent and Lewis, 'J. Physiol.,' vol. 36, p. 445; see also Brodie and Richardson, 'J. Physiol.,' vol. 21, 1897, p. 333, and 'Phil. Trans.,' B, vol. 191, 1899, p. 127; and also Vernon, 'J. Physiol.,' vol. 24, p. 239.

† Halliburton, *loc. cit.*; also Halliburton and Mott, 'Archives of Neurology," vol. 2.

‡ Demant, 'Zeitschr. f. Physiol. Chemie,' vol. 3, p. 241, and Kühne and Chittenden, 'Zeitschr. f. Biol.,' N. F., vol. 7, p. 358, 1889, have made some observations on this point, but I have been unable to consult the papers.



EXPERIMENTS.

Abstract.

Animal.	Experiment No.	Initial neg. var. 0·001 volt.	Nerve.	Extinction point.	Limits.	Notes.
Frog	B^p	2·0 q.p.	Sciatic	−8°·5 C.	−8°·5 C.	
Hedgehog*	C^{da}	0·12	Ext. popliteal	+6°·4	+ 6·4 to −1° 4	C^{de} was a 2nd experiment with the same nerve as C^{de}.
"	C^{de}	0·40	Int. popliteal	+2°·6		
"	C^{df}	0·44	"	−1°·3		
"	C^{de}	0·32	"	−1°·4		
Rabbit..........	B^{ub}	0·89	Int. popliteal	+7°·4	+7°·4 to +8°·8	
"	C^{de}	0·54	Ext. popliteal	+7°·1		
"	C^{db}	0·60	Int. popliteal	+8°·8		
Pigeon..........	C^{ea}	0·45	Median	+8°·2	+8°·2 to +6°·9	
"	C^{eb}	0·75	Median	+6°·9		

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* Dr. Pembrey very kindly furnished me with these animals in a state of hibernation.

sing the same apparatus that Waller* employed in his researches of e effect of temperature on the electrotonic current of frog's nerve.

The negative variation was observed in the way before mentioned. he value of the galvanometer deflection was ascertained by taking e scale value of 1 millivolt at intervals. It was found, as is well iown, that the resistance of the nerve and electrodes gradually creased as the temperature was lowered, and this causes a small ror in the strength of excitation, though this was annulled as r as possible by an added resistance of 100,000 ohms in the exciting rcuit.

Comments.

The limits determined are for the temporary abolition of the gative variation, not for its permanent abolition. There is a adual rise of the extinction point through the four classes of amphiians, hibernating mammals, mammals and birds. The limit varies a tle in each experiment in a manner that is not accounted for by her the apparent condition of the nerve or by its anatomical character. at some variation was to be expected was clear from the researches Howell,† who found that the vaso-constrictor fibres in the cat's atic were paralysed by cold (+4° C. *q.p.*), while the vaso-dilatator res were paralysed 1° lower, and even greater differences were the *lowest* point before extinction of the negative variation took place, and both the frog and hedgehog agree in giving a measurable response below 0°. In experiment B on the frog the negative variation reached a maximum at +3°·8 C. In the experiment C^{df} on the hedgehog an exactly similar maximum was observed at +7°·9 C., and plotting out the "cooling" and "warming" curves the latter gave also a maximum at the same point (fig. 7).

No such maximum was certainly observed in the mammal or pigeon. There were traces of a maximum at 25° C. in the former (in experiment B^{ub} and an earlier experiment on the kitten not recorded above), but the experiments C^{dg}, C^{dh}, C^{ed}, C^{eh} showed no sign of this. Several explanations are possible, but it seems preferable to await the result of further experiments before insisting too strongly on any of them. One, however, seems well established, that the negative variation follows the temperature with a certain "lag." This is seen to a small extent in the nerve of the hedgehog (fig. 7), in the rabbit and bird it is larger in amount, and tends to obscure curves taken in this way.

I have not yet determined the *permanent* extinction point, recovery took place in experiment B^{p} in the frog after a temperature of −3°·5 C. had been reached, and in Experiment B^{ub} on the rabbit (−2°·5), experiments are in progress in this direction.

The range of temperature through which the nerve can function is obtained by combining the figures here observed with those of the former series, and it is found that this range is the same for all the nerves examined, 45°·5 for the frog, 45°·2 for the rabbit, and 46°·1 for the pigeon, one step higher in the temperature scale in each case (Fig. 8).

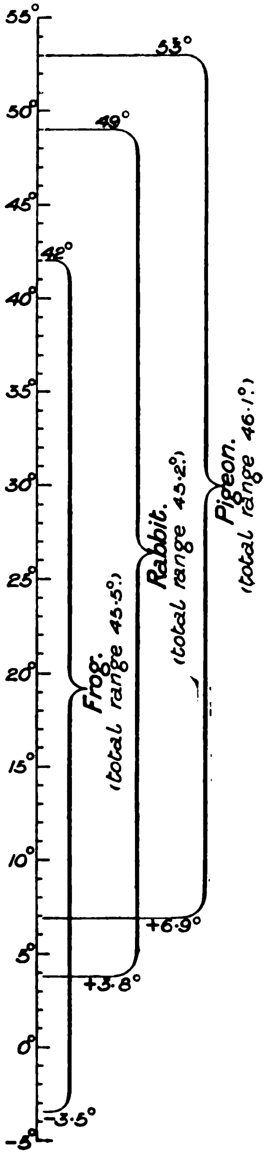

FIG. 8.

Conclusion.

(1.) It is possible to examine isolated mammalian and avian nerves under the same conditions as frog's nerves.

(2.) There is no essential difference between the nerves of frogs, mammals, and birds as regards their negative variation, excitability, and reaction to anæsthetics.

(3.) There is a marked difference in the extinction point for heat. The negative variation in frog's nerve is abolished at 40—42° C., in rabbit's nerve at 48—49°, in pigeon's nerve at 53°.

(4.) This extinction point corresponds closely with the first coagulation point of the body proteids, where these are known, and thus coagulation is probably the cause of the permanent loss of irritability of the nerve.

(5.) The point at which the nerves are paralysed by cold is -3°·5 in the frog, -1°·4 in the hedgehog, +3°·8 in the rabbit, and +6°·9 in the pigeon.

It gives me great pleasure to acknowledge my indebtedness to Dr. A. D. Waller for his great kindness and assistance in everything connected with this paper.

"On the Decline of the Injury Current in Mammalian Nerve, and exhibiting an actual rise, so that the highest value of the injury current is only attained when the nerve has lain for some time upon the electrodes. Several observers* have noted the initial rise, and explanations have been offered which accord with the generally held "alteration" theory of the nerve current. In face, however, of facts recently brought forward in a series of papers by one of us,† it is difficult to accept the prevailing theory as satisfactory. The attempt was therefore made to study this particular phase of the injury current from the point of view set forth in the papers alluded to. That view, which may be called the "concentration cell" theory of the injury current, is based upon the hypothesis of the core-model structure of nerve,‡ and lays stress on certain pre-existing peculiarities of constitution. These may briefly be described as (*a*) a separation of the solutions of electrolytes of the nerve into internal and external solutions by a membrane which permits only imperfect diffusion to take place between them; (*b*) a difference in the quantitative distribution of electrolytes in the solutions, the internal one being of small volume, but of great concentration and high specific conductivity. Such a difference between the solutions must give rise on rupture of the membrane, as by section or other injury, to diffusion processes, and consequently to differences of potential.

If such electrical differences as are found in the phenomenon of the injury current arise from the source indicated, they should be capable of modification in just the same manner that a process of diffusion can be modified. The value of a diffusion process depends primarily upon the concentration ratio of the two solutions in contact, and may be increased by diluting the weaker solution. In the experiments already reported upon,§ the extreme case of this dilution was exemplified by immersing the nerve for a short time in water; the result was an increase of the injury current due to the enhanced value of the diffusion process.

In relation to the experiments recorded here, it may be said that the temperature of the solutions, or the fact of any difference of temperature existing between them, is hardly less important than the concentration. In frog's nerve, the solutions are already approximately at the temperature of the laboratory. But with mammalian nerve the case is very different. Removed immediately after death, such a nerve has a temperature presumably not far removed from that of the ammalian body. Cooling must, therefore, continue after the nerve has ached the electrodes ; and this cooling is not a perfectly simple matter, r the fatty sheath of the nerve is not only, probably, a bad conductor of ectricity, but also undoubtedly an indifferent conductor of heat, e should expect, therefore, the cooling process to be differential, ie temperature of the external solution falling sooner than that the internal solution or axis cylinder ; and since cooling is a manner equivalent to dilution, the concentration ratio of the vo solutions should be at first increased, and with it the value the E.M.F.

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* Waller, "Croonian Lecture," 'Phil. Trans.,' London, 1896.

† J. S. Macdonald, "The Source of the Demarcation Current considered as a Concentration Cell," 'Proc. Roy. Soc.,' vol. 67, p. 315, &c., 1900, &c.; "The Injury Current of Nerve. The Key to its Physical Structure," 'The Thompson Yates Laboratories' Reports,' vol. 4, part 2, 1902, pp. 213—350.

‡ Grünhagen, 'Konigsberger Med. Jahrb.,' vol. 4, p. 199; Strong, 'Journal of Physiology,' vol. 25, p. 427; Boruttau, 'Pflüger's Archiv,' vol. 63, p. 154, &c., &c.

§ J. S. Macdonald, 'The Thompson Yates Laboratories' Report,' loc. cit.

The theory then affords an explanation of the increase observed hen a nerve just out of the body is examined at a lower temperaire. It remained to further exemplify this fact by attempting to produce the phenomenon at will. An experiment was so arranged iat the proper temperature of the nerve was artificially maintained, id then at a given moment allowed to fall gradually. The resulting irve shows that at or near body temperature the injury current eclined rapidly ; in this particular case it fell in half-an-hour 44 per ent. of its original value. In the second portion of the record *uring the period of cooling*, the rapid fall was arrested, there was a eriod of hesitation, and then a slow rise of the current. On the is reduced by exhausting the value of the source. A low temperature reduces the value of the injury current by checking the process upon which it depends. Both extremes, then, reduce the value of the current, but by quite different means. This being so, there will be a mean temperature around which these two effects are balanced; at which the value of the source decreases less rapidly than at the higher temperature, and the value of the diffusion process is greater than at the lower temperature, at which, therefore, the injury E.M.F. is best maintained. This consideration led to the systematic study of the injury current at different steady temperatures, and the sought-for point of best maintained E.M.F. was found to lie between 14° and 19° C.

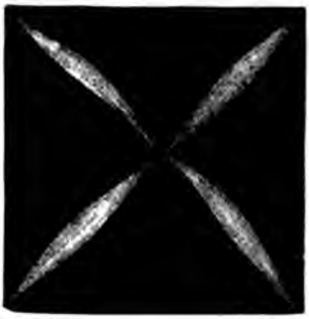
In the experiments hitherto considered, we have dealt with *current*, no allowance being made for changes of resistance brought about by altered temperature. The error, in some cases, was of no great moment, in others, those, namely, where a lowering of temperature gave an increased injury current; a correction for altered resistance would but have accentuated the point it was sought to establish. But there are instances in which the error might be serious—and it seemed, therefore, desirable throughout the inquiry to supplement the photographic records by a series of observations, in which measurements of potential by compensation should be taken at frequent intervals. Such measurements have been undertaken by us, and completely confirm the statements made above.

"On the Formation of Definite Figures by the Deposition of Dust." By W. J. Russell, Ph.D., F.R.S. Received January 29,—Read February 19, 1903.

(Abstract.)

The author shows that when a plate of glass or other material is slightly warmed and allowed to cool for 6 or 7 minutes in a dust-laden atmosphere, a clear and definite figure is formed on the plate. The figure is determined by the form of the plate on which it is deposited. If a square plate is used then a simple cross is formed, a ray of deposit proceeds from each corner of the plate to the centre. If the plate be triangular, a ray again proceeds from each corner; and with an octangular plate an eight-rayed star is formed. In every case the number and position of the angles of the plate determine the form of the figure. The dust generally used was that produced by burning magnesium ribbon, but any fine dust acts in the same way and produces the same figures.

With regard to the plate on which the figure is deposited, its com-sition is not of importance except as a back ground for the dust. glass plate for many reasons is best, but the figures form with equal



rtainty and sharpness on one of copper, or mercury, or ebonite, or dia-rubber, or card-board, &c. In order to heat the plate it may be ssed several times over the flame of a lamp, warming it as uniformly possible, and, if it be a glass plate, until the moisture condensed on e under side has disappeared; or the plate may be heated by laying on a copper plate heated to about 120°C. for 30 minutes, or it produced by placing a source of heat above the plate instead of below it. A large number of experiments are also recorded and illustrated showing the effect which different bodies in the immediate neighbourhood of the plate have on the figures which are formed. Taking only one case, that of a pin. When it is placed in contact and at right angles to the plate a definite deposit is produced, and this varies as the pin is moved further and further away, and as it is placed either on a level with plate, or above or below it. Even when the pin is 6 mm. below the level of the plate, and 2 mm. away from it, a distinct effect is produced. Again, these dust currents may be influenced in a remarkable way by suspending glasses of different sizes, and at different heights above the plate on which the figures are depositing, and photographs of the figures produced are given. The effects produced by obstructions of different sizes laid on different parts of the plate are also shown.

It was also found that a current of dust drawn through a tube will form a characteristic figure on a plate, which need not be warmed, as it passes over it.

If the magnesia dust be allowed to settle on a surface of water, about the temperature of 17° C. or, on water containing a very small amount of alcohol or glycerine, the deposit which forms on the surface breaks up, by the powder sinking, into a figure of cellular form.

Magnesia dust, which was generally used, undergoes some strange changes. When first deposited it is removed by the slightest touch, but if the plate be kept for a week or fortnight it may then be softly brushed over without damage to the figure. Another change which this powder undergoes is shown by collecting it immediately it forms, and examining it under a microscope, when it will be found to consist of irregular shaped and separate particles, but if the collection of the dust be made a few minutes after its formation, it is then seen that the particles are strung together, forming small and irregular fibres. In the various figures that have been produced the magnesia seems to have assumed this form.

"Mathematical Contributions to the Theory of Evolution.—On Homotyposis in Homologous but Differentiated Organs."

By KARL PEARSON, F.R.S., University College, London. Received January 20,—Read February 19, 1903.

(1.) In the paper on "Homotyposis in the Vegetable Kingdom,"* I defined homotypes as "undifferentiated like organs." In the course of that paper, I endeavoured to indicate that I was not unconscious of the influence of age, local environment, and position upon organism in modifying homotypic correlation. The object of my memoir, however, was to obtain some general appreciation of the average intensity of individuality in living forms, and to see if it approached the average value of fraternal heredity in plant or animal life. For this purpose I selected such material as was readily available, indicating the series where I thought differentiation of a sensible amount was present owing to the age, the situation, or the environment factors.

From the standpoint of theory, however, we are not compelled to adopt a mere indication of this kind. As soon as we can correlate between: (*a*) age and the quantitative character of the homologous organs, (*b*) situation on the organism and this same character, or (*c*) we are not dealing with homotypes at all, but with homologous parts, and we wish to reduce them to homotypes by correcting for differences between them due to growth or to situation on the organism.

I propose at present to deal only with problems (i) to (iii), not because (iv) does not admit of theoretical treatment, but because we have not thus far obtained data to illustrate satisfactorily the correlation between character and the immediate environment of the individual organism. Experimental determinations of homotyposis in plants, when the individuals are subjected to a graduated environmental scale, *e.g.*, in depth of soil or quantity of moisture allowed would be fairly easy to carry out, and most interesting in result. I hope it may be possible to arrange experiments of this kind for the coming summer. We can then illustrate the fourth proposition from actual observation, and the publication of its theoretical solution will be of greater value.

(2.) *To find the correction to be made to the apparent homotypic correlation when the pair of homologous parts are differentiated from each other by their periods of growth.*

Let x and y denote the characters in the two homologous parts quantitatively determined, and t_1, t_2 their respective periods of growth. Then we have four variable quantities x, y, t_1, t_2, no one of which fixes absolutely any other, for individuals will have different characters even with the same period of growth. The proposition accordingly reduces to this: What is the correlation R between x and y for constant values of the variables, *i.e.*, selected values of, t_1 and t_2?

This problem is answered in formulæ (lviii), (lix) and (lx) of my memoir: "On the Influence of Natural Selection on the Variability and Correlation of Organs."*

Let us write in those formulæ t_1 for the subscript 1, t_2 for 2, x for 3, and y for 4; we have at once

$$\Sigma_x^2 = \sigma_x^2 \frac{1 - r_{t_1t_2}^2 - r_{xt_1}^2 - r_{xt_2}^2 + 2r_{t_1t_2} r_{xt_1} r_{xt_2}}{1 - r_{t_1t_2}^2} \qquad \text{(i)},$$

$$\Sigma_y^2 = \sigma_y^2 \frac{1 - r_{t_1t_2}^2 - r_{yt_2}^2 - r_{yt_1}^2 + 2r_{t_1t_2} r_{yt_2} r_{yt_1}}{1 - r_{t_1t_2}^2} \qquad \text{(ii)},$$

$$\Sigma_x\Sigma_y\mathrm{R} = \sigma_x\sigma_y \frac{r_{xy}(1 - r_{t_1t_2}^2) - r_{xt_1} r_{yt_1} - r_{xt_2} r_{yt_2} + r_{t_1t_2}(r_{xt_1} r_{yt_2} + r_{yt_1} r_{xt_2})}{1 - r_{t_1t_2}^2} \qquad \text{(iii)}.$$

Now if we deal with direct and not cross-homotyposis, *i.e.*, with the *correlation* of the *same* character in two homologous parts, we can put these results more simply. We in this case render our correlation tables symmetrical by entering each one of a pair of homologues first as an x and then as a y. We may then write

---

* 'Phil. Trans.,' A, vol. 200, p 30.

$$r_{xt_1} = r_{yt_2} = r, \qquad r_{xt_2} = r_{yt_1} = r',$$
$$r_{xy} = \rho, \qquad r_{t_1t_2} = \mathbf{r}, \qquad \sigma = \sigma_x = \sigma_y,$$

and we find

$$\Sigma_x^2 = \Sigma_y^2 = \sigma^2 \frac{1 - \mathbf{r}^2 - r^2 - r'^2 + 2\mathbf{r}rr'}{1 - \mathbf{r}^2}$$
$$\mathrm{R} = \rho \frac{1 - \mathbf{r}^2}{1 - \mathbf{r}^2 - r^2 - r'^2 + 2\mathbf{r}rr'} - \frac{2rr' - \mathbf{r}(r^2 + r'^2)}{1 - \mathbf{r}^2 - r^2 - r'^2 + 2\mathbf{r}rr'} \ldots\ldots \text{(iv)}.$$

This is the full solution of the first problem.

We see that in order to solve it, it is necessary :

(i.) To find the correlation ρ of the homologous pairs as if they were simple homotypes.

(ii.) To find the correlation $\mathbf{r}$ between the growth periods of each pair of homotypes.

(iii.) To find the correlation r between the character and the period of growth.

(iv.) To find the correlation r' between the character of one homotype and the period of growth of its fellow.

Now these correlations can be found at once by the usual statistical processes, if the data are forthcoming.

(3.) I propose to illustrate this on material, which, although not

A few remarks must be made on these tables.

Table I gives the following values of the constants:—

Mean length of head of elder brother = 186·7508 in mm.
„ „ younger „ = 183·8296 „
Standard deviation of elder brother = 7·5027 „
„ „ younger „ = 7·3536 „

The correlation is, then, found to be 0·601,751,* and the regression, younger on elder brother, 0·5897. These give the intensity of heredity, uncorrected, for the growth factor.

Now, the most noteworthy part of this result is, as we shall see later, that *taking brothers at different ages tends to exaggerate the apparent intensity of heredity.* If we were to take pairs of boys at ages from 4 to 19, each pair having no hereditary relationship, but being, on the average, within a year or so of the same age, we should find a spurious correlation due to the mixture of material, each pair having approximately-like head-lengths because the members of it were, approximately, of like age. On the other hand, if the boys were blood relations of very different ages, their apparent relationship would be weakened, because we should be correlating the same organ at different stages of its growth. We have thus two factors: one tending to exaggerate, and the other to weaken the apparent strength of hereditary resemblance. It is of great interest to note that the former factor in the present case is the more effective.

In Table II we have what I term a growth table, *i.e.*, a correlation table between period of growth and the quantitative measure of a character. The constants of this table are as follows:—

Mean age of boy .... = 13·0394 years.
Standard deviation of age............ = 2·8207 „
Mean head-length .... = 185·4516 mm.
Standard deviation of head-length = 7·4991 „
Correlation of age and head-length = 0·453,496

The regression coefficient for head-length on age = 1·205676, and we have the probable head-length H_P for observed age A given by

$$H_P = 169 \cdot 7303 + 1 \cdot 2057\,A \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (\epsilon.)$$

Thus, on the average, boys' heads grow in length 1·2 mm. a year.

My results are based on 1637 cases entirely taken off the brother-brother data papers. Dr. Alice Lee at an earlier stage also worked out a growth table. We had not then so many brother-brother data papers filled in. She used in addition all the brother measurements on the brother-sister papers, and so reached 1856 boys, of which, I think, we may safely assert that 400 at least are not included in my series. She found: Mean age,* 12·7177; mean head-length, 184·8182; and slope of regression line, 1·2040, giving the formula

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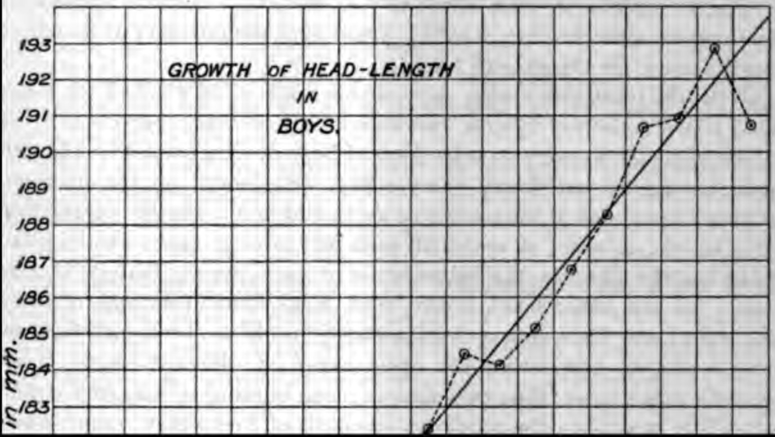
* Six figures have been kept in the correlation coefficients, as we require to calculate the regression coefficients from the differences of products and powers.

$$H_p = 169 \cdot 5061 + 1 \cdot 2040\,A,$$

a result in substantial agreement with mine.

Diagram 1.



line, until the range of data is very largely extended. The regression is sensibly linear.

Table IIIA and Table IIIB give the following results:—

Mean age of elder brother .. = 14·1249
S.D. of elder brother's age.. = 2·5124
Mean head-length, younger brother .... = 188·8578
S.D. of head-length, younger brother .... = 7·2806
Correlation of age of elder and head-length of younger .. = 0·396,598

Mean age of younger brother = 11·7149
S.D. of younger brother's age = 2·7221
Mean head-length, elder brother .... = 186·6515
S.D. of head-length, elder brother .... = 7·5005
Correlation of age of younger and head-length of elder . = 0·379,326

We see accordingly that within the limits of the probable error, the correlation between younger brother's head-length and elder brother's age is the same as that between elder brother's head-length and younger brother's age. This result might, to some extent, have been anticipated, but actual proof of this type of cross-relation is of value. In Table IV we have the correlation between ages of brothers giving the constants:—

Mean age of elder brother .... = 14·1508
Mean age of younger brother .... = 11·7487
S.D. of elder brother's age .... = 2·5080
S.D. of younger brother's age .... = 2·7220
Correlation of brothers' ages .... = 0·884,186

The first four results are in good agreement with those of Tables IIIA and IIIB. The last result shows how nearly there is an approximation to a constant difference in age between brothers in schools. Very closely we have—

Probable age of younger brother = 0·96 × (age of elder brother) − 1·83.

When the elder brother is 6, his younger brother is probably 2·1 years younger than he is; when the elder brother is 12, the younger brother is probably 2·3 years younger, and when he is 18, 2·6 years younger. The explanation of this is that when the elder brother is very young only his near or second brother will, as a rule, be at the same school, but in the secondary schools, which he reaches at a much later age, it is possible for a much younger brother to be at the same school.

Now let us substitute the correlation values, found in equations (i) to (iii), of page 290. We have

$$r_{xy} = 0\cdot601{,}654, \qquad r_{t_1t_2} = 0\cdot884{,}186$$

$$r_{xt_1} = r_{yt_2} = 0\cdot453{,}496,$$

$$r_{xt_2} = 0\cdot379{,}326, \qquad r_{yt_1} = 0\cdot396{,}598.$$

Whence we find

$$\Sigma_x/\sigma_x = 0\cdot890{,}051, \qquad \Sigma_y/\sigma_y = 0\cdot891{,}209,$$

and

$$\mathrm{R} = 0\cdot5446.$$

This is a very reasonable value of fraternal correlation, agreeing quite well with results obtained for horse, man and dog. It is worth noting that

$$r_{xt_1} \times r_{t_1t_2} = r_{yt_2} \times r_{t_1t_2} = 0\cdot4010,$$

and, therefore, either equals r_{xt_2} or r_{yt_1} fairly closely; in fact, within the probable error of their difference.

Hence, it would appear highly probable that the cross-relation between one brother's head length and a second brother's age is solely due to the correlation of the ages between the two brothers.

If such a result as

$$r_{xt_1} \times r_{t_1t_2} = r_{xt_2} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots \quad (\eta)$$

should be verified on the reduction of further data, it will enable us to much simplify our formulæ.

Thus we easily find for this case

Table I.—Collateral Heredity Head Length in Brothers, uncorrected for Age.

*Head-length of Elder Brother in mm.**

Head-length of Younger Brother in mm.

162—3.	162—3.	164—5.	163—7.	168—9.	170—1.	172—3.	174—5.	176—7.	178—9.	180—1.	182—3.	184—5.	186—7.	188—9.	190—1.	192—3.	194—5.	196—7.	198—9.	200—1.	202—3.	204—5.	206—7.	208—9.	Totals.
162—3	—	—	2	1	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	3
164—5	—	—	—	—	—	—	—	—	1	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	2
166—7	—	—	—	—	1	1	0·5	1·5	—	—	—	—	—	—	—	—	0·25	0·25	—	—	—	—	—	—	4·5
168—9	—	—	1	0·5	1·5	1	1	1	2	1	1	1	1	1	—	—	0·25	0·25	—	—	—	—	—	—	13·5
170—1	1	—	—	4	3	3	—	3	4	2	3	3·5	3	3	—	—	1	—	—	—	—	—	—	—	33·5
172—3	—	—	—	1·75	4·25	2	2	3	1	2	2	2·5	1	1	—	1	—	—	—	—	—	1	—	—	24·5
174—5	—	—	1	0·75	3·25	1	—	5	5	9	3·5	8	2	5	—	1	1	—	—	—	—	—	—	—	45·5
176—7	—	—	—	—	1	1	6	3	8	5·5	10·5	10	4	3	2	—	1	—	1·5	—	—	—	—	—	56·5
178—9	—	—	—	—	2	3	2·5	6·5	11	8	5	11·5	8	5·5	4·5	2	3	1	1·5	—	—	—	—	—	75
180—1	—	—	—	5	1·5	—	2	4	11	17	13·5	14·5	16	14	6·5	8·5	6	2·5	—	—	1	1	—	—	119·5
182—3	—	—	—	—	2	2	2·5	4·5	8	9	13	14·5	8·5	12	11	10	2	1·75	1·25	1	—	—	—	—	103
184—5	—	—	—	—	—	—	—	2	1	11·5	12	17	15·5	17·75	9·75	8·25	7·25	4·25	3·25	2	2	—	—	—	113·5
186—7	—	—	—	—	—	1	2	2	2	6	12·5	7	8	15·25	17·75	11·75	8·25	5	1	—	—	—	—	—	99·5
188—9	—	—	—	1	—	1	1·5	1·5	2	4	5	7	6	9·5	12	6·5	7·5	4·5	1	1	1	—	—	—	72
190—1	—	—	—	—	—	—	—	—	—	2	1	3·5	8·5	10·5	9·5	7·5	10·5	10	3	2	—	2	—	—	70
192—3	—	—	—	—	—	—	—	—	2	1	2	2	4	7	10·5	7·5	8	6	7	1·5	—	1	—	1	60·5
194—5	—	—	—	—	—	—	—	—	—	1	1	1	1	5·5	4	3	2·5	5	5	3·5	1	1	—	—	34·5
196—7	—	—	—	—	—	—	—	—	—	—	—	1	1	1	3·5	2	5	0·5	3·5	—	—	1	—	—	18·5
198—9	—	—	—	—	—	—	—	—	—	—	—	—	2	—	—	2	2	4	2	4	2	1	—	—	19
200—1	—	—	—	—	—	—	—	—	—	—	—	—	1	—	1	—	—	1	1	2	1	1	1	—	9
202—3	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	1	2	—	—	4
204—5	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1·5	0·5	—	—	1	—	—	—	—	—	3
206—7	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	1
208—9	—	—	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	—	—	1
Totals	1	0	4	9·5	19·5	16	20	37	58	79	85	104	92·5	112	93·5	71·5	65·5	47	32	17	9	11	1	1	986

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* The group 162—3 contains all boys with head-lengths greater than 161·5 and less than 163·5 and so on. When a head-length fell exactly on such a value as 183·5 it was halved between the columns 182—3 and 183—4.

*Boy.**

13.	14.	15.	16.	17.	18.	19.	Totals.
—	—	1	—	—	—	—	3
—	—	—	—	—	—	—	2
—	—	—	—	—	—	—	6·5
0·5	—	1	—	—	—	—	16
5·5	3	1	—	1	—	—	39·5
5	1	2	—	—	—	—	38·5
2·5	3·5	3	1	1	—	—	49
12	4·5	4	1	—	—	—	77
11·5	9	5	5	1	—	—	103
27	18	9·5	8	5	—	—	161
25	17	16	6	2	3	2	157·5
36	25	23	13·5	6	1	1	192·5
24	17·5	13·5	3·5	8	1	—	158
20	22·5	17	19·5	9	9	1	162·5
18·5	19·5	19·5	16	9	4	0·5	137·5
18·5	17·5	18	10·5	6·5	6	4·5	112·5
8·5	3	18·5	15·5	6·5	4	1	77
7·5	11	7	9	4	6	1	54·5
3	7	8	4·5	12	1	1	42·5
—	2	5	5·5	3	1	—	19·5
1	1	1	3·5	1	1	—	9·5
1	—	2	6	2	1	—	12
—	—	—	1	—	1	—	3
—	—	1	—	1	—	—	3
227	182	176	129	78	39	12	1637

day would be placed in the *n*th column, or the mean age of such

Table IIIA.—Correlation of Age of Elder and Head-length of Younger Brother.

*Year of Age of Elder Brother.**

Head-length of Younger Brother in mm.†	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	Totals.
162—3	—	—	—	—	—	—	—	1	1	—	—	—	—	—	—	—	2
164—5	—	—	—	—	—	—	—	—	1	—	1	—	—	—	—	—	2
166—7	—	—	—	—	1	—	1	1	1	0·5	—	—	—	—	—	—	4·5
168—9	—	—	—	—	—	2	1	6	2	2·5	1	—	—	—	—	—	14·5
170—1	—	1	1	—	2	1·5	2	5·5	7·5	6	1·5	3	1	—	—	—	32
172—3	—	—	—	—	2	0·5	4·5	3·5	6	2·5	2·5	2	2	—	—	1	26·5
174—5	—	—	—	—	1·5	3	4·5	3	10	7·5	6·5	2	3	1	—	—	42
176—7	—	—	—	—	1·5	2	8	8	6·5	10	4·5	8	2	2·5	—	—	53
178—9	—	—	2	—	1	2	10	9	15·5	11	8	5	8	2·5	—	—	74
180—1	—	—	—	—	1	5	14	12	20·5	16	16	14	9·5	7·5	5	2	122·5
182—3	—	1	—	—	2	4	5	7	14	19	8	17	12	8·5	1	3	101·5
184—5	—	—	—	—	—	—	5·5	5	16·5	15·5	18	21	17·5	9·5	5	2	115·5
186—7	—	—	—	—	—	2	4·5	9	20	19	10	15	8	3·5	7	—	98
188—9	—	—	—	—	1	3	2	7	8	16	7·5	5·5	10	10·5	5	1	76·5
190—1	—	—	—	—	—	—	—	4	6·5	10·5	12·5	9·5	7	11·5	5	1	67·5
192—3	—	—	—	—	—	—	3	1	3	10	8	9	13	7·5	5	3	62·5
194—5	—	—	—	—	—	—	1	—	4	3	5	3	5	3·5	5	4	33·5
196—7	—	—	—	—	—	—	—	—	3	2	2	6	4	2	—	—	19
198—9	—	—	—	—	—	—	—	—	—	2	2	3	6	3	1	1	18
200—1	—	—	—	—	—	—	—	1	—	1	—	—	1	2	2	—	7
202—3	—	—	—	—	—	—	—	—	—	—	—	—	1	1	1	—	3
204—5	—	—	—	—	—	—	—	—	—	—	—	1	1	1	—	1	4
206—7	—	—	—	—	—	—	—	—	—	—	1	—	—	—	—	—	1
208—9	—	—	—	—	—	—	1	—	—	—	—	—	—	—	—	—	1
Totals..	—	2	3	—	13	25	67	83	146	154	115	124	111	77	42	19	981

* See Note to Table II.

† See Note to Table I.

Elder and Age of Younger Brother.

*er Brother.**

12.	13.	14.	15.	16.	17.	18.	19.	Totals.
—	1	—	—	—	—	—	—	1
—	—	—	—	—	—	—	—	0
—	1	—	—	—	—	—	—	4
1·5	—	—	—	—	—	—	—	8·5
3·5	1	—	1	—	—	—	—	18 5
2	2	—	—	—	—	—	—	16
3·5	2	1	1	—	—	—	—	20
4·5	1	2	1	—	—	—	—	37
6	4	3	3	—	—	—	—	57
9	8	6	3	—	—	—	—	71
8	14·5	5	2	1	2	—	—	89
20·5	6·5	10	4·5	4	—	—	—	108
12	10	4	3·5	2	—	—	—	92·5
12·5	16·5	7	11	8	—	—	—	112
10	12·5	12·5	9	1	1·5	—	—	89
7	11	6·5	11·5	5·5	1·5	1	—	71·5
6·5	9	12	9·5	4·5	2	—	—	68
5·5	9·5	7	10	2	1	—	—	45
2	2·5	9	7	3	1	—	—	32
1	2	2	6·5	—	—	—	—	14
1	1	2	1·5	1	—	—	—	8
3	1	6	—	1	—	—	—	12
—	—	2	—	—	—	—	—	2
—	—	—	1	—	—	—	—	1
119	116	97	86	33	9	1	—	977

† See Note to Table I.

Table IV.—Correlation of Ages of Pairs of Brothers.

*Years of Age of Elder Brother.**

Year of Age of Younger Brother.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	14.	15.	16.	17.	18.	19.	Totals.
4	—	1	—	—	1	—	1	1	—	—	—	—	—	—	—	—	4
5	—	1	2	—	3	—	3	2	2	—	—	—	—	—	—	—	13
6	—	—	1	—	4	4	5	5	1	—	—	—	—	—	—	—	20
7	—	—	—	—	4	10	10	14	8	8	2	2	—	—	—	—	58
8	—	—	—	—	—	10	26	13	16	6	1	—	—	—	—	—	72
9	—	—	—	—	—	1	17	35	32	16	7	2	1	1	—	—	112
10	—	—	—	—	—	—	1	17	55	26	8	3	2	—	—	—	112
11	—	—	—	—	—	—	—	—	28	58	25	11	7	—	—	—	129
12	—	—	—	—	—	—	—	—	3	41	39	16	11	6	—	1	117
13	—	—	—	—	—	—	—	—	—	—	30	60	14	6	2	2	114
14	—	—	—	—	—	—	—	—	—	—	5	27	43	18	7	5	105
15	—	—	—	—	—	—	—	—	—	—	—	2	32	32	12	5	83
16	—	—	—	—	—	—	—	—	—	—	—	—	1	17	14	2	34
17	—	—	—	—	—	—	—	—	—	—	—	—	—	1	5	5	11
18	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	1	1
19	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	0
Totals	—	2	3	—	12	25	63	87	145	155	117	123	111	81	40	21	985

* See Note to Table II.

This table contains only fifteen pairs of twins, one pair of brothers having been born within ten months of each other. Any age, as 9, means all falling within the ninth year, *i.e.*, from the ninth birthday to the day before the tenth birthday, so that two brothers, not twins, might appear of the same age.

(4.) *To find the correction to be made to the apparent homotypic correlation, when each pair of homotypes is differentiated by a common period of growth from other pairs of homotypes.*

The solution of this problem may be deduced at once from equation (iii) of the preceding problem by simply putting $t_1 = t_2$. In this case $r = r'$, $\mathbf{r} = 1$, and we find

$$\mathrm{R} = \frac{\rho - r^2}{1 - r^2} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad \text{(v)}.$$

This equation was given by me in a note in *Biometrika*, vol. 1, p. 404, and its use illustrated on Dr. Simpson's data for *Paramecium caudatum.*

(5.) *To find the correction to be made to the apparent homotypic correlation when the pair of homologous parts are differentiated from each other by situation on the organism.*

We have only to put in formula (iii) on p. 290, $t_1 = p_1$ and $t_2 = p_2$, the positional co-ordinates of the first and second homologous parts, to make that formula available for position instead of age differentiation. If we denote by c_1 and c_2 the characters of the parts in the positions p_1 and p_2 respectively, our solution takes the form below, where we have confined our attention to the same character, Let $\bar{p}$ = mean position, σ_p = standard deviation of positions on the arbitrary position scale. Let there be n organisms, and suppose that S_m denotes a summation of all m homologous parts on an organism, and S a summation for all n organisms. Then, if $\sigma_p = \sigma_{p_1} = \sigma_{p_2}$,

$$\begin{aligned} nm(m-1) r_{p_1p_2} \sigma_p^2 &= S_n \{S_m (p_1 - \bar{p})(p_2 - \bar{p})\} \\ &= S_n \{S_m (p_1 - \bar{p}) \times S_m (p_2 - \bar{p}) - S_m (p_1 - \bar{p})^2\} \end{aligned}$$

But $S_m (p_1 - \bar{p}) = 0$, hence, since $S_m (p_1 - \bar{p})^2 = m\sigma_p^2$,

$$nm(m-1) r_{p_1p_2} \sigma_p^2 = -nm\sigma_p^2,$$

or

$$r_{p_1p_2} = -\frac{1}{m-1} \qquad \text{(vii).}$$

Further

$$\begin{aligned} nm(m-1) r_{p_1c_2} \sigma_p \sigma_c &= S_n \{S_m (p_1 - \bar{p})(c_2 - \bar{c})\} \\ &= S_n \{S_m (c_2 - \bar{c}) \times S_m (p_1 - \bar{p}) - S_m (c_1 - \bar{c})(p_1 - \bar{p})\} \\ &= -S_n \{S_m (c_1 - \bar{c})(p_1 - \bar{p})\}. \end{aligned}$$

But $$S_n \{S_m (c_1 - \bar{c})(p_1 - \bar{p})\} = nm\, \sigma_c \sigma_p\, r_{c_1p_1}.$$

Hence

$$r_{p_1c_2} = -\frac{r_{c_1p_1}}{m-1} = r_{c_1p_1} \times r_{p_1p_2} \qquad \text{(viii),}$$

a relation precisely similar to that discovered in the case of growth periods for brother's head-lengths from the actual numbers on p. 294.

Substituting we find

$$\begin{aligned} 1 - r_{p_1p_2}^2 - r_{p_1c_1}^2 - r_{p_1c_2}^2 + 2r_{p_1p_2} r_{p_1c_1} r_{p_1c_2} &= (1 - r_{p_1p_2}^2)(1 - r_{p_1c_1}^2) \\ 2r_{p_1c_1} r_{p_1c_2} - r_{p_1p_2}(r_{p_1c_1}^2 + r_{p_1c_2}) &= r_{p_1p_2} r_{p_1c_1}^2 (1 - r_{p_1p_2}^2) \end{aligned}$$

Then substituting in (vi) and using (vii) we determine the simple formula for homotyposis corrected for positional differentation

$$R = \frac{\rho}{1 - r_{pc}^2} + \frac{1}{m-1} \frac{r_{pc}^2}{1 - r_{pc}^2} \qquad \text{(ix).}$$

where r_{pc} stands for the correlation of character and position on the organism.

An exactly similar formula might be found for the correction for the age or growth factor, if the m homologous parts dealt with had the same distribution of ages or growths in each organism.

(7.) Now the equation just found has the serious disadvantage that it is based on the linearity* of the regression relation between position and character. But while organic and homotypic correlations give for a surprising variety of cases sensibly linear regression relations, the relation between position and mean character is far more rarely linear. We obtain, as a rule, remarkably smooth curves. We, therefore, require some modification of equation (ix).

---

* The reader should note that this condition does not involve any assumption of normal frequency, or the Gaussian law. The latter applies only to a very special case of linear regression.

Still supposing the regression of character and position linear, we should have, if σ' be the mean standard deviation of an array of organs in the same position,

$$\sigma'^2 = \sigma^2(1 - r_{pc}^2).$$

But if σ_{M} be the standard deviation of the means of the arrays, we have from first principles

$$\sigma^2 = \sigma_{\mathrm{M}}^2 + \sigma'^2$$

Hence $$\sigma_{\mathrm{M}} = \sigma \times r_{pc}.$$

We can now write equation (ix) in the form

$$\mathrm{R} = \rho \frac{\sigma^2}{\sigma^2 - \sigma_{\mathrm{M}}^2} + \frac{\sigma_{\mathrm{M}}^2}{(m-1)(\sigma^2 - \sigma_{\mathrm{M}}^2)} \text{ .... (x).}$$

This is quite free from r_{pc}, and, what is more, although we have deduced it from (ix) and the relation $\sigma'^2 = \sigma^2(1 - r_{pc}^2)$ peculiar to linear regression, it is now free of any limitation as to the nature of the

Or, noting that

$$\bar{c} = S_m S_n(c_1)/(nm) = S_m(\bar{c}_p)/m, \text{ we have}$$

$$\sigma'^2 = \sigma^2 - \sigma_M^2,$$

where σ' is the standard deviation of the character-individualities free from the position factor. We see that it is precisely the same quantity as we have previously used for the mean standard deviation of the arrays for given positions.

Next taking the correlation of characters c'_1 and c'_2 in positions p_1 and p_2 we have

$$S_m(c_1'^2) + S_m(c_1'c_2') = S_m(c_1^2) + S_m(c_1 c_2) - 2S_m(c_1)m\bar{c} + m^2\bar{c}^2.$$

To get this result we have multiplied every quantity like $c'_1 = c_1 - \bar{c}_{p_1}$ by every other quantity like $c'_2 = c_2 - \bar{c}_{p_2}$ *and by itself*, and then added such quantities together for every position on the one organism. Thus on the left hand side there are m terms in the first, $m(m-1)$ terms in the second summation; on the right hand side there are m terms in the first, $m(m-1)$ terms in the second and m terms in the third summation. Now sum for each of the n organisms, and we have

$$nm\sigma'^2 + nm(m-1)R\sigma'^2 = mn(\sigma^2 + \bar{c}^2) + nm(m-1)(\rho\sigma^2 + \bar{c}^2) - 2m^2n\bar{c}^2 + m^2n\bar{c}^2.$$

Whence

$$R = \rho\sigma^2/\sigma'^2 + \frac{\sigma^2 - \sigma'^2}{(m-1)\sigma'^2},$$

or, as before

$$R = \rho\frac{\sigma^2}{\sigma^2 - \sigma_M^2} + \frac{\sigma_M^2}{(m-1)(\sigma^2 - \sigma_M^2)} \quad \ldots\ldots\ldots\ldots\ldots \text{(x)}.$$

Now while this proof is independent of the theory of partial correlation coefficients, involving only simple algebra, and is further independent of any consideration of linear regression, it yet wants something of the width of the former theory, which allows us at once, for example, to correct for a combination of factors, such for example as for *both* growth and position influences simultaneously. The difficulty lies entirely in the extent within which it is legitimate to assume the relation between position or age, and the mean value of the character at that position or age to be linear. It is therefore clearly advisable to start by plotting this relationship,* and fitting, if possible, such position or growth graphs with appropriate curves. If, for the series of positions dealt with or the period of growth taken, we find that a straight line† is a close approximation to the relationship, then we may use the general theory of partial correlation, otherwise we must fall back on results like (x). For example, in head growth in boys, we cannot much improve on a straight line; in positional influence on the branches in the whorls of *Equisetum arvense* we need at least a third order parabola.

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* In the case of some animals and many plants the relationship is in itself of much interest, for it expresses a law of development or growth in serial parts.

† The analytical consideration of this point is very simple. If the regression

(8.) Although material for several investigations on the homotyposis of serial homologous parts has been collected, the progress in some of these cases is slow, as it involves rather laborious microscopic measurement. I content myself at present with an illustration from the vegetable kingdom.

I collected in the autumn of last year, 126 plants of *Equisetum arvense* in Raydale Side, an offshoot of Wensley Dale; the plant was growing on a lane side high up above Semmerwater. This *Equisetum* grows from the top with a single stem, and I counted the number of branches to the whorl from the root upwards. As a rule, there will be one or two whorls close to the soil which have never developed any branches at all; then we have what I shall term the *first* whorl in which some branches have developed, but the number is irregular and obviously subject to some cause of variation, other than the growth law of the plant. The number of branches to the whorl then increases uniformly and steadily up to the 4th whorl, after which it falls almost equally when gathered at least as far as the 12th or 13th whorl, and I doubt whether even beyond this so late in the season, any further branching would have taken place. A few branches were broken off, and these were of course counted; there was no difficulty, however, in easily ascertaining whether a branch had in any case been developed or not, and the peculiarity of the 1st whorl was certainly not due to missing, but to undeveloped branches.

Table V gives the relation between branches to the whorl and position for the whole of the 126 plants. In two columns to the right are given the means and variabilities of the branches for each whorl.

Now, whether we judge by mean or standard deviation, we see a perfectly gradual change from whorl to whorl, which absolutely precludes us from considering the number of branches to the whorl as a pure homotypic character. We see a marked differentiation due to position of the whorl on the plant; the whorls are homologous but not homotypic parts.

Suppose, however, that we disregard our test for differentiation,* and proceed to find a correlation table for the whole material as homotypic. We have Table VI, for which I have to heartily thank Dr. Alice Lee.

The value found for the homotypic correlation from this table is

$$\rho = -0\cdot0064 \pm 0\cdot0185,$$

or, there is no sensible homotyposis at all.

But we might have gone to the other extreme and taken only the 3rd, 4th, and 5th whorls, which have more nearly the same means and standard deviations as homotypes. The result is Table VII, giving

$$\rho = 0\cdot7918 \pm 0\cdot0129.$$

It will be perfectly clear, therefore, as these two results ought to be the same, if the whorls were true homotypes, that we may get any result at all if we neglect differentiation.† The answer to this is that no trained biometrician would call these whorls "undifferentiated like organs" with the two right hand columns of Table V before him.

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* On the test for differentiation, see 'Biometrika,' vol. 1, p. 334.

† Bateson, 'Roy. Soc. Proc.,' vol. 69, p. 200.

V.

the Whorl.

9.	10.	11.	12.	13.	Totals.	Mean.	Standard deviation.
22	17	11	—	—	126	7 619	2·360
37	40	16	3	—	126	9·294	1·273
35	45	23	5	—	126	9·627	1·187
33	45	28	3	1	126	9·730	1·151
35	41	30	2	—	126	9·643	1·158
35	38	24	3	—	124	9·427	1·363
28	29	17	1	—	123	8·732	1·781
24	14	4	—	—	121	7·297	2·291
10	5	—	—	—	119	5·555	2·553
1	—	—	—	—	110	3·964	2·199
—	—	—	—	—	97	2·443	1·506
—	—	—	—	—	67	1·866	0·960
—	—	—	—	—	39	1·462	0·746
—	—	—	—	—	12	1·333	0·471
—	—	—	—	—	4	1·250	0·433
—	—	—	—	—	2	1·000	0·000
260	274	153	17	1	1448	—	—

Table VI.

Number of Branches on 1st Whorl of Pair.

Number of Branches on 2nd Whorl of Pair.

	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	Totals.
1	132	110	63	55	41	61	91	120	281	274	146	22	3	1399
2	110	98	49	49	57	54	90	129	281	237	160	18	1	1333
3	63	49	16	18	14	31	59	79	122	149	55	6	—	661
4	55	49	18	12	19	24	53	82	103	121	55	1	1	593
5	41	57	14	19	18	22	67	45	115	109	58	5	—	570
6	61	54	31	24	22	8	54	70	133	154	54	7	—	672
7	91	90	59	53	67	54	162	122	199	173	72	4	2	1148
8	120	129	79	82	45	70	122	300	281	262	111	28	2	1631
9	281	281	122	103	115	133	199	281	766	401	150	9	1	2842
10	274	237	149	121	109	154	173	262	401	914	240	25	2	3061
11	146	160	55	55	58	54	72	111	150	240	428	40	1	1570
12	22	18	6	1	5	7	4	28	9	25	40	34	—	199
13	3	1	—	1	—	—	2	2	1	2	1	—	—	13
Totals	1399	1333	661	593	570	672	1148	1631	2842	3061	1570	199	13	15692

7. 2

Table VII.—Whorls, 3rd, 4th, and 5th only.

Number of Branches to 1st Whorl of Pair.

Number of Branches on 2nd Whorl of Pair.	7.	8.	9.	10.	11.	12.	13.	Totals.
7	28	8	8	1	1	—	—	46
8	8	28	19	3	—	—	—	58
9	8	19	146	26	6	—	1	206
10	1	3	26	196	35	1	—	262
11	1	—	6	35	110	9	1	162
12	—	—	1	—	9	10	—	20
13	—	—	—	1	1	—	—	2
Totals	46	58	206	262	162	20	2	756

Now let us consider how to handle the material, allowing for the ifferentiation of the whorls. To begin with, our formula requires he use of the same number of homologous parts for each organism, nd it is, on account of the value of the probable error of the random ample, undesirable to use fewer than 100 individuals. This leads to ur cutting off Table V at the 10th whorl. In this way we get rid lso of the forking, which certainly begins in many individuals at the 1th or 12th whorl. Table VIII gives us the data of Table V recon-

Table VIII.—Relation between Number of Branches and Position of Whorl.

Number of Branches in Whorl.

Position of Whorl.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.	13.	Totals.	Means.
1	2	2	3	8	8	6	9	24	20	17	11	—	—	110	7·7182
2	—	—	—	—	1	3	4	16	32	38	13	3	—	110	9·3273
3	—	—	—	—	—	—	7	6	29	42	21	5	—	110	9·7182
4	—	—	—	—	—	—	4	7	27	43	25	3	1	110	9·8273
5	—	—	—	—	—	—	6	7	28	39	28	2	—	110	9·7455
6	—	—	—	—	1	3	5	7	31	37	23	3	—	110	9·5636
7	—	—	—	3	2	5	9	10	27	28	16	1	—	110	8·8909
8	1	2	5	5	5	11	19	20	24	14	4	—	—	110	7·4909
9	5	10	11	12	8	14	19	16	10	5	—	—	—	110	5·7364
10	18	20	13	11	17	14	11	5	1	—	—	—	—	110	3·9636
Totals 2—10	24	32	29	31	34	50	84	103	209	246	130	17	1	990	8·2515

ll suffice to demonstrate this heterogeneity. Certain individuals have e normal number of about 8·5 branches to this whorl, but about a fifth the total number of individuals only develop about half this normal mber of branches. To illustrate this I have in Diagram 2 plotted e mean number of branches to the whorl, and fitted these means ith a parabola of the third order,* using only whorls 2 to 10. The uation to this parabola is

$$y = 9{\cdot}451,443 - 0{\cdot}549,4302x - 0{\cdot}179,9881x^2 - 0{\cdot}008,4206x^3,$$

e origin being at the 6th whorl, and y giving the mean number of anches for x whorls from the 6th. We have the following results:—

Position of whorl.	Observed number of branches.	Calculated.
1	7·718	8·752
2	9·327	9·308
3	9·718	9·707
4	9·827	9·898
5	9·746	9·829
6	9·564	9·451
7	8·891	8·714
8	7·491	7·565

3,938,354. Further, if σ be the standard deviation of the frequency distribution of branches, as found from the bottom row of Table VIII, we have

$$\sigma^2 = 6{\cdot}721{,}083.$$

Hence for use in formula (x) we have, since $m = 9$,

$$\frac{\sigma^2}{\sigma^2 - \sigma_{\mathrm{M}}^2} = 2{\cdot}415{,}280, \quad \frac{1}{m-1}\,\frac{\sigma_{\mathrm{M}}^2}{\sigma^2 - \sigma_{\mathrm{M}}^2} = 0{\cdot}176{,}911 \;\ldots\ldots\; \text{(xi)}.$$

Table IX gives the uncorrected homotyposis for the nine whorls treated as simple homotypes. From this I find,

$$\rho = 0{\cdot}131{,}258 \;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\; \text{(xii)}.$$

Substituting (xi) and (xii) in (x), we find for the homotyposis of the number of branches in the whorls in *Equisetum arvense*, when corrected for differentiation due to position,

$$\mathrm{R} = 0{\cdot}4939.$$

This result it must be admitted is extremely satisfactory, and indicates how it is quite possible to correct a result like (xii) by allowing for the differentiation of the homologous parts due to serial position.*

I hope before long to publish other results dealing with homotyposis in serial parts, where the differentiation has every variety of intensity. I think they will suffice to show that differentiation is not a subtle and evasive quality beyond the appreciation of the naturalist who is provided with the training requisite for modern biometric research.

(9.) The values of R as given by (ix) and (x) may be illustrated from the actual numbers for *Equisetum arvense*. We have seen in the footnote, p. 304, that

$$\eta = \sigma_{\mathrm{M}}/\sigma.$$

This in our case gives

$$\eta = 0{\cdot}76549.$$

But by direct calculation on Table VIII, using whorls 2 to 10, Dr. Lee finds $r_{pc} = -0{\cdot}64616$. Hence with the notation of the footnote referred to

$$\sigma' = 0{\cdot}7632\,\sigma, \qquad \Sigma_{\mathrm{M}} = 0{\cdot}4104\,\sigma.$$

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* The value obtained for the crude homotyposis of the members of the whorls in *Asperula odorata* in my first memoir was $\rho = 0{\cdot}1783$ ('Phil. Trans.,' A, vol. 197, p. 326). I have little doubt that when we are able next summer to calculate the correction for differentiation in position of whorl, we shall find R for woodruff in good accordance with other homotypic results. My remarks about it were: "In counting the members on the whorls I soon found evidences of differentiation in position, the whorls towards the top of the spray having, as a rule, fewer members than those lower down" (*loc. cit.*, p. 325). Unfortunately I have not kept my records of position.

typosis for *Equisetum arvense.*

st *Whorl of Pair.*

7.	8.	9.	10.	11.	12.	13.	Totals.
19	22	37	39	10	5	—	192
21	29	75	54	13	—	—	256
21	33	59	56	14	2	—	232
30	35	56	49	22	—	1	248
31	22	79	55	38	3	—	272
38	39	96	113	40	4	—	400
10	70	131	139	57	3	2	672
70	144	142	169	96	22	1	824
31	142	562	304	121	9	1	1672
39	169	304	770	197	21	2	1968
57	96	121	197	398	33	1	1040
3	22	9	21	33	34	—	136
2	1	1	2	1	—	—	8
72	824	1672	1968	1040	136	8	7920
84	103	209	246	130	17	1	990

Thus η diverges much from r_{pc} and Σ_M from zero. Indeed, a glance at the diagram shows how far we are from true linear regression. If we use r_{pc} as above instead of η, *i.e.*, (ix) instead of (x), we have

$$R = 0{\cdot}3511,$$

a value very much below the actual value. This illustration will suffice to emphasise the importance of testing the actual curve of regression before we assume it to be linear and use equation (ix).

(10.) The subject of differentiation due either to position or age is, of course, a difficult one, but it does not seem at all beyond biometric treatment. The greatest difficulty which it seems to me will have to be encountered is not that of discovering and allowing for differentiation due to serial position, but in ensuring that when this has been allowed for, there is not remaining an organic correlation due to the necessity of adjacent parts "fitting." On this account it is most desirable that as large a number of homotypes as possible shall be taken, so that the part of the correlation due to the homologous parts having to fit, or, indeed, to serve a common end, should be reduced to as small a quantity as possible. For example, if we suppose adjacent whorls to have their number of branches influenced by an organic relationship, this result will only bias nine out of the forty-five pairs we should form in dealing with ten whorls. The question of separating organic from homotypic correlation is one that I hope to return to at a later date. Meanwhile the present paper will suffice to indicate how partial correlation coefficients enable the biometrician to free himself from the differentiation between individuals due to different periods of growth, or to different positions on the organism.

In conclusion I should like to thank Dr. Alice Lee and Mr. F. E. Lutz for aid readily granted at one or other stage of this investigation.

'rimitive Knot and Early Gastrulation Cavity co-existing with Independent Primitive Streak in *Ornithorhynchus*."

By Professor J. T. WILSON, M.D., and J. P. HILL, D.Sc., University of Sydney. Communicated by Professor G. B. HOWES F.R.S. Received January 21,—Read February 12, 1903.

Amongst the material at our disposal for the investigation of the lier stages in the development of the Monotremes is an egg of nithorhynchus, measuring 10 × 9·5 mm. The stage of development presented by this egg is of such interest and importance that we ve deemed it deserving of a brief preliminary communication.

The stage of embryonic organisation would appear to fit in as mediately succeeding the oldest of the early embryos described by mon,* viz., his "E_6" (fig. 15, Taf. 8). His "E_7" appears, indeed, have been older than "E_6," but with the exception of one figure g. 39, Taf. 9) representing the structure of the extra-embryonic stoderm, Semon gives no indication of the conditions met with in it ovum. This is the more to be regretted, as we see reason to lieve that "E_7" must have corresponded pretty closely with the membrane, and distended by a considerable bulk of fluid material. It is quite impossible to remove the shell without serious damage to the delicate blastoderm. Fixation with the shell intact is imperative, and, as a matter of fact, the result in the way of preservation proves quite satisfactory, as evidenced by the condition of the cellular blastoderm, in which mitotic figures are well preserved. But even after fixation, the opening up of the ovum is attended with no little risk. The inevitable evacuation of the contained fluid allows of crumpling of the blastodermic membrane with possibility of injury to the embryonic area. It is impossible, owing to its size and osmotic difficulties, to treat the ovum throughout unopened. Even were that course practicable, the impossibility of orientation would be an insuperable difficulty.

After fixation and subsequent dehydration in graded alcohols, the 10 mm. egg was cleared in origanum oil and opened. The blastoderm still remaining *in situ* in relation to the shell was examined from the interior aspect, and was found to possess at one spot a small more opaque area, somewhat oblong, but rather irregular. The portion of the blastoderm containing this small opaque patch was photographed by transmitted light at a magnification of 6·5 diameters for the purpose of orientation.

Our surmise that the area in question was of the nature of an embryonic or primitive knot was afterwards confirmed by the examination of serial sections.

No differentiation in way of an embryonic area in the wider sense is recognisable in the photograph, nor was any such discovered in the course of examination of the wall of the blastodermic vesicle *in toto* under low magnification. We naturally concluded that the very evident knot represented the earliest and only differentiated area, and for a time devoted our attention solely to this area and the blastoderm in its vicinity. The portion of the blastoderm containing the knot was separated from the remainder of the wall of the vesicle, and was then imbedded and cut in serial sections. Examination of these did in fact show that in the neighbourhood of the knot, and for some distance from it, the wall of the vesicle was destitute of any indication of further differentiation. But towards the periphery of the portion sectioned and comparatively remote (nearly 2 mm.) from the knot, itself, we found the commencement of a region of thickened ectoderm with underlying mesoderm. Our attention thus being directed to other manifestations of developmental activity in the blastoderm in addition to the primitive knot, we found, in the portion of the vesicular wall originally put aside, a quite extensive area showing important changes. These amount to no less than the establishment, quite away from the region of the knot, of a distinct linear primitive streak formation, surrounded by an area over which the ectodermal

er is thickened and cubical, and within which a mesodermal sheet
already undergone a wide extension.

t is peculiarly unfortunate that, owing partly to the difficulty of
ling with the delicate and originally collapsed and torn vesicular
ll after the cutting open of the egg, partly to the failure to detect
visible differentiation apart from the knot, and our consequent
viction that the latter was the sole trace of embryonic organisation
present, the orientation of the distinct portions into which the
icle was separated was not adequately determined and preserved.
thus became impossible for us to guarantee that the planes of
tioning of the remaining portions should be accurately co-ordinated
h each other, or with that of the important piece first sectioned.
is failure, not wholly blameworthy, when the conditions of the task
appreciated, has introduced an element of conjecture into our sub-
uent attempt to determine the precise relation to the knot of the
mitive streak area, of whose existence we later on became aware.
vertheless, we think that we shall be able to establish these rela-
ns with at least a high degree of probability.

n this preliminary paper we propose to restrict ourselves to an
ount of the highly interesting area which first attracted our atten-
n, and which we have already referred to as a "primitive knot."
will, we think, be admitted that no possible doubt can be entertained

the larger diameter of the embryonic knot, and parallel to its presumable axis, on the supposition that a clear spot near one of its margins represented the position of the gastrula opening or blastopore, thus determining the true posterior margin of the knot.

Examination of the series did ultimately show that the sections were approximately longitudinal sections through the gastrula cavity.

The general character of the egg at the stage under consideration must be briefly referred to. At the period now dealt with, the formerly yolk-laden ovum has become transformed into a large blastodermic vesicle with fluid contents, amongst which are large numbers of dispersed yolk-spheres of the original yolk-mass. One can, without hesitation, homologise the interior of the vesicle with the subgerminal cavity of a Sauropsidan egg, extended so as to include by liquefaction the whole of the yolk itself. Ornithorhynchus indeed may be said to afford an actual demonstration of the transformation of a Sauropsidan subgerminal cavity, such as is figured in its first beginnings in Semon's figures 36 and 38 of his "O_3," into the cavity of a mammalian blastodermic vesicle, thus supporting Keibel's view of the correspondence of these cavities.

The establishment of the vesicular stage has been effected by the extension of the cellular blastoderm completely around the yolk, a condition representing a considerable advance on that existing in Semon's figures "E_5" and "O_3." He does not record the condition in this respect of his "E_6." He was also unable fully to determine the condition of "E_7" in this respect. We believe that the latter must have been very similar to our present stage, judging from the figure he gives of a portion of the extra-embryonic blastoderm.

Not only is the cellular wall of the blastodermic vesicle complete in our specimen, but it is already bilaminar throughout, and trilaminar over a not inconsiderable area. A complete layer of yolk-entoderm ("secondary" entoderm) has been differentiated lining the cavity, except under the small area of the primitive knot, with whose tissue it is continuous. The yolk-entoderm cells are large and swollen, being distended by yolk-spheres of various sizes and somewhat different staining capacities. Owing to their yolk-laden character their protoplasmic contents are relatively greatly reduced. The nuclei are, as a rule, large and vesicular. In the least successful sections the entoderm cells may be broken up and more difficult of recognition. We are firmly convinced that it is owing to the imperfection of the section shown in Semon's figure 39 from his "E_7" that such a careful observer has been unable to recognise the yolk-entoderm as such, and figures it as a zone of vacuolated coagulum next the surface of the yolk. The imperfect layer of angular-looking cells which he figures and regards as the entoderm in "E_7" is not really such, but forms an intermediate layer of mesodermal cells.

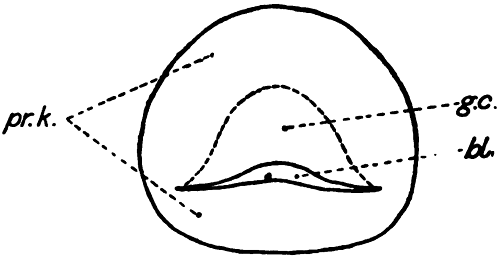
This layer of mesoderm is well represented in the stage under escription. But it by no means extends throughout the whole extent f the blastodermic vesicle, nor is it met with at all at or in the nmediate vicinity of the primitive knot. It is found most fully eveloped in and around the primitive streak area, and here it is ontinuous with the ectodermal thickening of the primitive streak .*e.*, paraxially), where it attains greater thickness and consists of everal layers of cells. Outwards from the line of the primitive treak* it thins out gradually into a single layer, and further out ecomes patchy and incomplete. It is throughout distinct and inependent of the underlying yolk-entoderm, which can be followed hrough the whole wall of the vesicle. We have several prearations of small portions of the wall of the vesicle stained and ounted *in toto*, which give clear demonstration of the huge yolk-laden ntoderm cells forming a continuous lining of the vesicle wall, precisely s figured by Hill and Martin† in a considerably later stage.

The ectoderm of the vesicle wall throughout the greater part of its xtent forms a membrane consisting of very thin flattened cells closely pplied to the vitelline membrane. This character it retains in the icinity and over a portion of the primitive knot. But over the egion already referred to as the primitive streak area, remote from osterior to) the primitive knot and extending outwards so as to be in size with that now described. It is to be regretted that Semon's specimen was lost through accident, so that no further comparison can be instituted. There is, however, a general resemblance between fig. 15 of Semon's E_6 and his figs. 14 and 18 showing areas from other eggs of Echidna (E_5) and Ornithorhynchus (O_3). The dimensions of these can be calculated and are both found somewhat to exceed those of our primitive knot, in spite of their relatively younger ages. It is therefore difficult to say to what extent the areas referred to (of E_6 and E_5 and O_3) correspond to our primitive knot. We are, however, of opinion, that E_6, though probably younger, must have pretty closely corresponded, and there can be little doubt but that the centre of the other areas, at least, is later on developed into an actual primitive knot. The sectional figure of O_3 shown in fig. 38, interpreted by Semon as showing the commencement of gastrulation, may possibly bear this interpretation, but we cannot feel satisfied that the granular coagulum layer between the cellular layer and the surface of the yolk shown in this figure has been adequately interpreted, especially in view of our complete conviction that in fig. 39 the layer represented in somewhat similar fashion is beyond all doubt really yolk-entoderm.

We do not wish to be taken as suggesting that here in O_3 the coagulum represents differentiated yolk-entoderm. We are simply not convinced beyond all doubt that all the formed cell elements of this area in O_3 are disposed in the form of a superficial continuous membrane as shown in fig. 38, more especially in view of the totally different condition of the nearly related stage E_5, as illustrated in the sectional fig. 33, and of the condition described below in our own somewhat later stage in Ornithorhynchus.

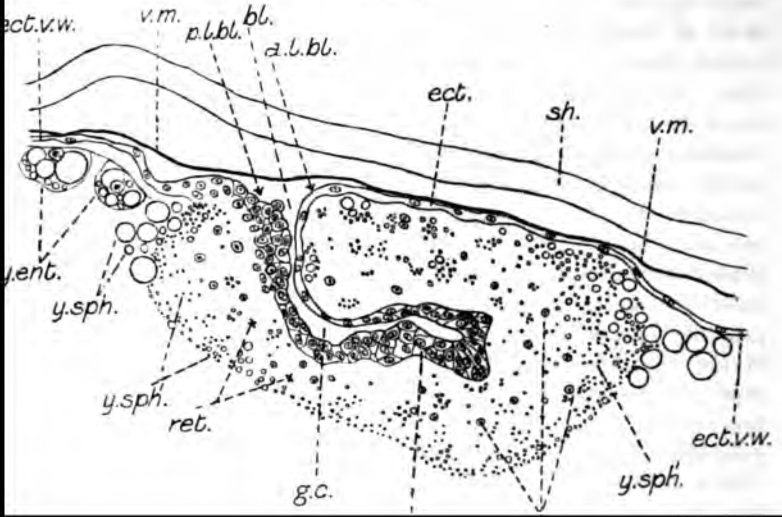
The general form of the embryonic knot has already been noted. A schematic surface projection of its outline is shown in fig. 1. This figure represents a plane reconstruction of the area of the knot, compiled from the serial antero-posterior sections through the region. One of the more typical of these sections is also figured semidiagra-

Fig. 1.



matically in fig. 2. More adequate illustration by photomicrography and otherwise will be forthcoming in a future communication.

FIG. 2.



front without change of character as far as the transversely elongated blastopore seen in fig. 1 and in section in fig. 2. Thence it is continued round the anterior lip of the blastopore so as to line the roof of the blastoporic (archenteric) canal or gastrula-cavity.

Posteriorly to the knot the thin ectoderm is continued forwards, but on reaching the posterior edge of the knot, its cells lose their markedly flattened character, and become continuous with a mass of cuboidal or even rounded cells, which forms the prominence of the posterior lip of the blastopore. The superficial cells of this mass do not, however, wholly surrender their epithelial arrangement, and are continued forwards in the hinder wall and floor of the gastrula-cavity to complete the cellular lining of this cavity. This cellular lining consists, for the greater part of its extent, of large and cuboidal cells, but in the hinder part of the roof, and extending from this upwards around the anterior lip of the blastopore, the cells are more flattened, ultimately becoming continuous with the thin ectoderm overlying the anterior part of the knot.

Figs. 1 and 2 together enable one to realise the form of the gastrula-cavity as a wide, but dorso-ventrally flattened cavity opening posteriorly by a transversely-elongated blastoporic aperture.

The posterior lip of the blastopore is formed by a mass of cells which, if not wholly indifferent, at least show a less clear differentiation into superficial and deep than is found elsewhere.

As was indicated at the outset, the resemblance between the primitive knot in Ornithorhynchus and that found in a number of reptilian forms is very striking. We would particularly draw attention to the figures by Mitsukuri* of the primitive knot in Chelonia, and more especially to his figs. 9 and 13 on Plate 8, which show a remarkable similarity to the condition here described, in all essential features. In Ornithorhynchus we lack entirely the columnar arrangement of the ectoderm over the knot, and the anterior lip of the blastopore there shown, otherwise the characters both of the knot and of the gastrula-cavity and its wall appear to be almost identical with those figured, especially in Mitsukuri's fig. 9, Plate 8.

In a future more extended communication we hope to illustrate more adequately the points above set forth, and in addition to add something in the way of elucidation of the fate of the primitive knot in somewhat later stages. We propose also to describe and illustrate the condition of the primitive streak area, which at the period now dealt with already co-exists with, though independently of, the primitive knot, but which later by extension comes into more intimate relationship with the knot.

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* K. Mitsukuri, "On the Process of Gastrulation in the Chelonia," 'Journ. Coll. Sci., Japan,' vol. 6. *Cf.* also Gertrude C. Davenport, 'Radcliffe Coll. Monograph,' No. 8. Boston, 1896. Dendy, 'Qu. Jl. Micro., Sci.,' vol. 42, p. 18, 1899.

The mere fact of its co-existence at this stage with the knot, necessarily occasions some reconsideration of the morphological relationship of the mammalian primitive streak to the process of gastrulation.

"The Brain of the Archæoceti."[*]

By G. ELLIOT SMITH, M.A., M.D., Fellow of St. John's College, Cambridge, Professor of Anatomy, Egyptian Government School of Medicine, Cairo. Communicated by Professor G. B. HOWES, LL.D., D.Sc., F.R.S. Received January 15,—Read February 12, 1903.

So far as I have been able to ascertain, nothing whatever is known of the form of the brain or, more strictly, of the cranial cavity in the Archæoceti. Hence no apology is needed for presenting even this imperfect account of two cranial casts representative of this sub-order, which have come into my hands.

Among the Eocene remains found in the Fayûm region of the Egyptian desert by Mr. H. J. L. Beadnell and Dr. Charles W. Andrews, in 1901, there was a broken skull of *Zeuglodon*,[†] from which [redacted] several peculiarities of the natural cast—such, for example, as the form of the caudal part of the cerebellum and the shape of the cerebral hemispheres—which are difficult to reconcile with the artificial mould, even if we admit that the inner face of the cranium (from which the latter was made) is damaged or imperfectly cleaned. The differences, nevertheless, are sufficiently pronounced to indicate a generic distinction between the two specimens; and in this connection it is interesting to recall a statement made by Dr. Andrews in his first reference to *Zeuglodon*, as "including apparently Dames' *Z. Osiris*, and perhaps a second species."* It would, however, be very unwise, because it would serve no useful purpose, and possibly lead to error, to found a new genus or even a new species on the evidence of this natural cranial cast, when our source of information concerning the known genus (*Zeuglodon*) is as unsatisfactory as that obtainable from the artificial one about to be described. More especially so, when it is remembered that in the case of the only indisputable facts (*i.e.*, size and general form) the two casts are agreed. I shall therefore merely describe and attempt to explain the meaning of the form of the two specimens, and leave the question of the specific identity open for future research.

The general appearance of the brain is extraordinarily peculiar (figs. 1 and 2). The shape of the anterior part of the natural cast (fig. 1, *a* and *b*) closely resembles the cerebrum of a Lizard greatly magnified. An anterior prismatic stalk (*a*), representing the pedunculi olfactorii, suddenly expands into a plump, broad, smooth mass (*b*), showing the form of the chief part of the cerebrum. The maximum breadth of the two hemispheres (fig. 1, *b*) is 95 mm.; the greatest length of each (measured in front from the point where the ventral surface of the olfactory peduncles appear to expand into the chief mass of the hemisphere) is 47 mm.; and the maximum depth is 54 mm. Each cerebral hemisphere (exclusive of the olfactory peduncle) is slightly broader than it is long.

The two olfactory peduncles are represented in the natural cast by a single prismatic process. This extends forward for a distance of 37 mm. (measured along the dorsal edge) in front of the point where the expansion to form the hemispheres commences; and as the peduncles are broken across there, it is not possible to estimate their total length or the shape and size of the olfactory bulbs.

The coronal section formed by its anterior (broken) surface gives an isosceles triangle with a base measuring 8·5 mm. and sides of 10 mm. each. It expands as it passes backward, so that at its junction with the rest of the hemisphere its sides are each 19 mm. and its base 16 mm. in length.

In the artificial cast (fig. 2) all that represents this extensive olfactory stalk is an irregular rostrum with two small boss-like projections, one above the other (*a* and *a'*). The cerebral hemispheres in the natural cast have a broad base, from which the sides extend upward toward the narrow dorsal surface with a gradual slope. In the artificial cast, however, the lateral parts of the hemispheres seem to be expanded into full rounded swellings.

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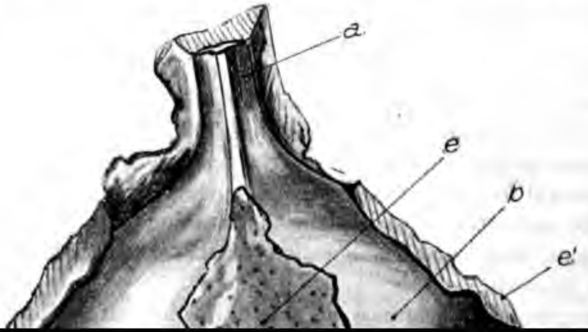
* 'Geological Magazine,' September, 1901, p. 401.

Then, again, the antero-posterior diameter of the hemisphere is much shorter (being about 13 mm. less) than it is in the natural cast, although the breadth of the two specimens is approximately the same.



there is (in the natural cast) a large, irregular mass of a very peculiar shape, not exactly comparable to the condition occurring in any other brain known to me.

Immediately behind the hemispheres (*b*) there is a great transverse bar (*c*) measuring 125 mm. in the transverse direction—*i.e.*, extending on each side 15 mm. beyond the lateral margin of the cerebrum (*b*).

Each lateral extremity of this mass (*c*) is expanded to form a large buttress. In the natural cast these buttress-like masses are practically vertical, and of uniform thickness; whereas in the artificial cast they are obliquely-placed, and expanded ventrally. In the natural cast the mesial continuation of these thick lateral masses (each of which measures 30 mm. antero-posteriorly) becomes reduced to a bridge measuring only 5 or 6 mm. [the exact figure cannot be stated, because a piece of bone (fig. 1, *e*) partially covers this region].

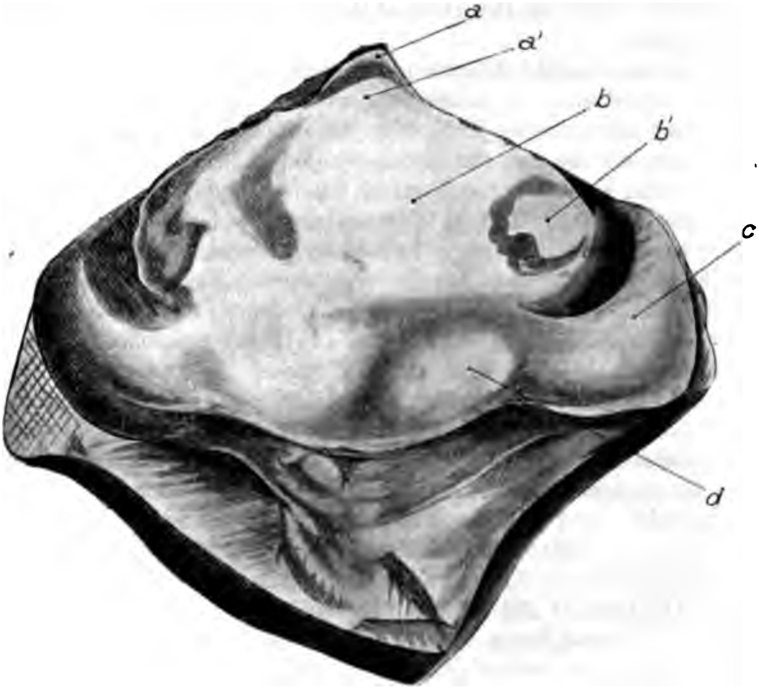


FIG. 2.—Dorsal aspect of the artificial cranial cast of *Zeuglodon*. ⅗ natural size. *a*, *b*, *c*, *d*, as in fig. 1. *a'*, the dorsal rostrum, and *b'*, an irregular boss on the cerebral hemisphere. (These are probably due to imperfections in the cranium.)

In the deep concavity behind the narrow bridge of the area *c* (in the natural cast) two rounded, irregular, walnut-like bosses project, one on each side of the middle line (fig. 1, *d*). Each of these is 26 mm. in diameter, and is placed so obliquely that its surface looks almost directly backward. Shallow but clearly defined furrows separate these two bodies from each other and from the area *c*. In the artificial cast there is only a very faintly-marked indication of these bodies (fig. 2, *d*).

At a first glance it might seem that they represent the whole cerebellum, in which case *c* would be part of the cerebrum! But careful examination of the natural cast renders such an interpretation highly improbable, and comparison with the artificial cast seems to finally establish the belief that the whole of the region marked *c* forms part of the cerebellum.

It is extraordinarily difficult to accurately interpret this peculiar form of cerebellum. A comparison with other primitive types of cerebellum* points to the probability that the lateral buttresses of the mass *c* represent the floccular lobes, and that the walnut-like mass (*d*) represents the cerebellar lobule which I have called "area C" (*op. cit.*, 'Catalogue,' p. 211). If it be objected that the lateral buttress-like mass is much too extensive to be entirely "floccular," attention may be called to the fact that in the large aquatic Sirenia, which have retained an exceedingly primitive type of brain, the floccular lobes are enormous in comparison with those of other mammals (*op. cit.*, 'Catalogue,' p. 346).

brain dwindle to very small proportions. In the Odontoceti the olfactory bulb and its peduncle actually disappear. The Archæoceti, therefore, are subject to two factors, which will account in some measure for their small cerebrum. For, in addition to the smallness of the brain to which most Eocene mammals are subject, there is their aquatic mode of life. This causes a reduction in size of just those portions of the pallium which form the greater part of the Eocene hemispheres.

In the modern Cetacea the neopallium attains to the greatest absolute size which it ever reaches in any mammal. This fact cannot, however, be considered fatal to the belief in the close affinity of the Archæoceti and the Cetacea, because the extraordinary dissimilarity between the brains in the two sub-orders is such as we know to have been produced by the operation of well-recognised causes in the long lapse of time which separates the dawn of the Tertiary period from the present day. In all mammals which lead a life "in the open" it has become a condition of their survival that the neopallium must increase in size in each successive generation: failing this, the creature must either adopt a "retired and safe mode of life" or become extinct. Numerous examples might be quoted in support of this hypothesis. But the case of the Sirenia shows us how little we really know of the factors which determine the size of the brain. These creatures began the struggle for existence in Eocene times with relatively large brains, in spite of their aquatic mode of life; and they have been succeeded by generations of descendants whose latest progeny at the present day have a brain-equipment only slightly superior to their earlier Tertiary ancestors (*vide* 'Catalogue,' *op. cit.*, p. 344, *et seq.*). Even if we admit that the modern Manatees and Dugongs lead an eminently safe and retired life, which is in marked contrast to the venturesome and "open" life of the whales and porpoises, much still remains to be satisfactorily explained.

Perhaps the most striking feature of the brain of *Zeuglodon* is the extreme disproportion between the size of the enormous cerebellum and the diminutive cerebrum. In this respect the fossil brain presents a most marked contrast to that of all recent mammals, and especially to that of the Cetacea. This relatively great size of the cerebellum is not peculiar to the Archæoceti, but is common to other extinct mammals of large size. In my memoir on the brain in the Edentata* the difficulty presented itself of adequately explaining a similar phenomenon in *Glyptodon*; and it must be borne in mind, in even attempting to do this, (1) that the obtrusive greatness of the cerebellum presents itself only in large mammals and not in lowlier vertebrates, and (2) that the size of the cerebellum is not proportionate o that of the cerebrum. In the case of *Glyptodon* I four years ago ttempted to explain these facts in this manner.

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* "The Brain in the Edentata," 'Linnean Society's Trans.,' 2nd series, Zoology, vol. 7, part 7, 1899, p. 381.

The development of the neopallium in mammals opens up the ossibility of the performance of many more complex muscular acts han are possible in the Amphibia or Reptilia: these acts require a co-ordinating mechanism, the size of which will be largely determined by the bulk of the muscular masses, the actions of which are to be harmonised, and the extent of the sensory surfaces which send into he cerebellum streams of controlling impulses. A large cerebellum s being demanded by a large mammalian body, even if the cerebrum s small. I cannot offer any more satisfactory explanation of the magnitude of the cerebellum in *Zeuglodon* than this.

It is clear from the foregoing that the extraordinarily great contrast n the appearance of the brain of the Archæoceti and that of the Cetacea cannot be urged as a reason against their kinship, when it is emembered that the operation of known factors is quite sufficient o explain the transformation of the one type into the other in the ime which has separated the Eocene period from the present.

Having disposed of these negative arguments, we may consider the ositive evidence for Cetacean affinity in the brain of *Zeuglodon*.

The shape of the cerebrum, and especially its relatively great readth is peculiar. In fact this form of hemisphere rarely or never

Cetacea. There are no characters of the brain of the modern Cetacea which can be regarded as certainly distinctive, if we put aside such features as the extreme dwindling of the olfactory apparatus, and the enormous development of the neopallium. Both must be regarded as late acquisitions, not to be expected in an Eocene mammal. Under these circumstances these slight points of positive evidence of the relationship of the Archæoceti and Cetacea must be allowed some value, as reinforcing the testimony of the skeletal parts.

If we seek to institute closer comparisons between the brain of *Zeuglodon* and of the Odontoceti and Mystacoceti with a view to the determination of its relationships, we are not unnaturally doomed to disappointment. It might, perhaps, be supposed by some anatomists that the absence of an olfactory bulb in the Odontoceti might point to a closer affinity of *Zeuglodon* to the Mystacoceti, in which a small olfactory apparatus is retained. But there is every indication that the olfactory apparatus of the Odontoceti has become aborted quite recently.

Thus in a specimen of the embryonic brain of the Narwhal (*Monodon*), which was given to me some years ago by Professor Howes, the remains of the olfactory bulb (fig. 3, *b.o.*) are still quite visible as a small umbilicate area in part of the "desert region" of Broca (fig. 3, *a.d.*), wherefore it follows that in the early embryo the olfactory bulb and peduncle develop as in all other mammals. Moreover, in all Odontoceti traces of the pyriform lobe are found even in the adult; and in the brain of *Kogia Greyi* the rhinal fissure and the typical (macroscopically only) pyriform lobe are retained in a form as clearly defined as that of any macrosmatic mammal (fig. 4). Professor Haswell, in describing this brain* emphasises the fact that "the most remarkable feature of

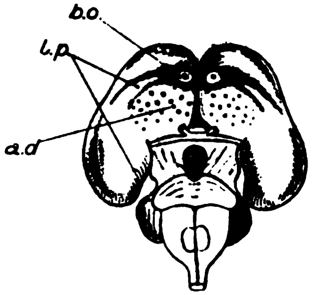

FIG. 3.—Ventral aspect of brain of an early fœtus of *Monodon*. Natural size. *a.d.*, locus perforatus (desert region); *b.o.*, bulbus olfactorius; *l.p.*, lobus pyriformis.

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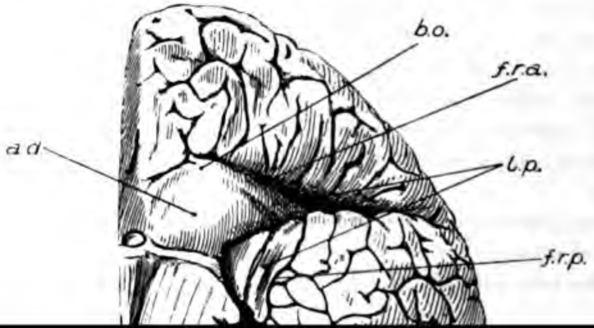
* W. A. Haswell, "On the Brain of Grey's Whale (*Kogia Greyi*)," 'Linnean Society of New South Wales Proc.,' vol. 8, 1883 (publ. 1884), pp. 437—439, pl. XXI.

the [basal] region, and perhaps of the whole brain, is the great depth of the ectorhinal fissure, a feature marking off the present form very strongly from *Delphinus*" (p. 438). Since his illustrations do not properly delineate this interesting conformation, Professor Haswell kindly permitted me to examine his specimen; and Mr. J. P. Hill has made me an excellent photograph (of its ventral surface), roughly reproduced in the accompanying drawing (fig. 4). It shows the complete and quite-typical rhinal fissure and the characteristic pyriform lobe. In its anterior part the rhinal fissure is fully a centimetre deep.



is 410 c.c. If the necessary corrections and estimations be made from this gross cubic capacity, the weight of the brain in the Archæoceti must have been considerably less than 400 grammes, and perhaps nearer 300, as against that of the recent Cetacea, which ranges from 455 grammes in *Kogia* (Haswell) to 4,700 grammes in *Balænoptera* (Guldberg).

"The Differential Invariants of a Surface, and their Geometric Significance." By Professor A. R. FORSYTH, Sc.D., F.R.S. Received February 14,—Read March 5, 1903.

(Abstract.)

The present memoir is devoted to the consideration of the differential invariants of a surface; and these are defined as the functions of the fundamental magnitudes of the surface and of quantities connected with curves upon the surface which remain unchanged in value through all changes of the variables of position on it. They belong to the general class of Lie's differential invariants; and some sections of them were obtained about ten years ago by Professor Zorawski, who, for this purpose, developed a method originally outlined by Lie. Earlier, they had formed the subject of investigations by a number of geometers, among whom Beltrami and Darboux should be mentioned.

Professor Zorawski's method is used in this memoir. In applying it, a considerable simplification proves to be possible; for it appears that, at a certain stage in the solution of the partial differential equations characteristic of the invariance, the equations which then remain unsolved can be transformed so that they become the partial differential equations of the system of concomitants of a set of simultaneous binary forms. The known results of the latter theory can then be used to complete the solution.

The memoir consists of two parts. In the first part, the algebraic expressions of the invariants up to a certain order are explicitly obtained; in the second, their geometric significance is investigated.

An invariant, which involves the fundamental quantities of a surface E, F, G, L, M, N (these determine the surface save as to position and orientation in space) and their derivatives up to order n, as well as the derivatives of functions ϕ, ψ, of position on the surface up to order $n+1$, may itself be said to be of order n. The invariants up to the second order inclusive are obtained. It appears that, if two functions ϕ and ψ occur, all the invariants that occur up to the second order can be expressed algebraically in terms of

29 algebraically independent invariants; while, if only a single function ϕ occurs, all the invariants that occur up to the second order can be expressed in terms of 20 algebraically independent invariants.

The significance of these respective aggregates of 29 and of 20 invariants is obtained in connection with curves

$$\phi = 0, \psi = 0,$$

drawn upon the surface. The investigation reveals new relations among the intrinsic geometric properties of a curve upon a surface. In particular, up to the second order, four such relations exist for a single curve; and their explicit expressions have been constructed.

"The Electrical Conductivity of Solutions at the Freezing-point of Water."

By W. C. D. WHETHAM, F.R.S., Fellow of Trinity College, Cambridge.

Received February 14,—Read March 5, 1903.

The following paper contains an account of experiments which bring to greater concentrations the series of measurements on the conductivities of dilute solutions at the freezing point, communicated to the Royal Society in February, 1900.* The work has been carried on at intervals during the last two years, and was made possible by the kindness of Professor Ewing, who placed a room at the Cambridge Engineering Laboratory at the disposal of the writer and his wife.

The earlier experiments were originally planned in connection with the observations undertaken by Dr. E. H. Griffiths on the freezing points of corresponding solutions; they were therefore conducted in a platinum cell of design similar to that used by Griffiths, with the object of eliminating any solvent action of glass. Any such action would be quite inappreciable at the concentrations used in the experiments now to be described; resistance cells of glass were consequently used, and the labour of observation was much reduced.

The structure of the cells is shown in figs. 1 and 2; that illustrated in fig. 1 was made of Jena glass, and reserved for the more dilute solutions. Each cell is so arranged that by applying a slight exhaustion, the whole of the contents can be drawn up into a bulb; by this means, when water or stock solution is added, complete mixture can be easily secured. The method of preparing the solutions was usually the same as that formerly employed; the pure solvent was placed in the cell, and its weight and resistance determined; weighed quantities of stock solution were then added successively, the resistance being measured after each addition. When, however, the solutions approached saturation, this procedure was reversed; the stock solution was first examined, and was then diluted by a weighed quantity of water.

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* 'Phil. Trans.,' A, vol. 194 (1900), p. 321.

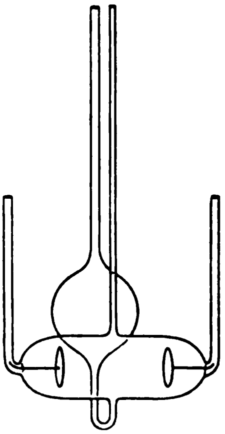

FIG. 1.
(¼ full size.)

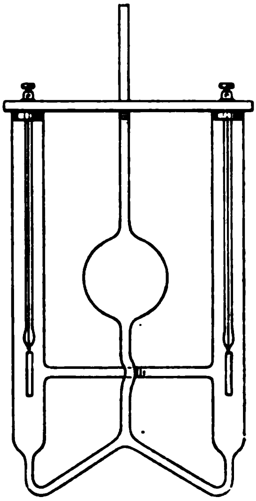

FIG. 2.
(¼ full size.)

The resistance cell was placed in a tin bath filled with melting ice. At first, a thermometer was inserted in the cell, and the observations postponed till its readings became steady at 0° centigrade; but it was soon found that the resistance itself gave a more sensitive and convenient means of thermometry. Measurements were taken at intervals till they became constant; the cell being always completely immersed in ice, the result thus obtained shows the resistance at 0°. As an example, the following numbers may be given:—

Time.	Resistance.
12.45	2179
12.55	2191
1.12	2202
1.16	2202
1.18	2202

It was intended to make up the solutions in the resistance cell while it was immersed in the ice, but condensation of water from the atmosphere was found to occur on the inside of the cold vessel when stock solution was added. Before each addition, therefore, the cell and its contents were raised to some temperature near that of the room by standing the cell for a few minutes in slightly warmed water.

The water used as solvent was first boiled, then distilled in glass from alkaline permanganate, and finally redistilled in a platinum still with a trace of acid potassium sulphate. It was kept in a large stoppered flask of Jena glass till required for the experiments.

The samples of salt used to make the stock solutions of potassium chloride and copper sulphate were the same as those prepared for the earlier experiments; details will be found in the account of those experiments to which reference has already been made. In the cases of barium chloride, potassium bichromate, and magnesium sulphate, the best salt, sold as chemically pure, was obtained, the two salts first mentioned being recrystallised before use. Any probable impurities would not affect the results to an amount equal to the other errors of experiment.

The measurement of the electrical resistance was performed exactly as in the earlier set of experiments. The current from one or two dry cells was alternated by means of a revolving commutator, which was driven by a hand wheel and cord, the connections of a D'Arsonval galvanometer being simultaneously alternated by the same instrument. The alternating currents were passed through a Wheatstone's bridge, Potassium Chloride I. KCl = 74·59.

In Cell No. 2, the water used had a resistance of 8·3 × 10^6 ohms at 0°.

m.	$m^{\frac{1}{3}}$.	R.	$k/m = \mu$.	μ/μ_∞.
0·02393	0·288	7894	755	0·936
0·04261	0·349	4504	744	0·922
0·07924	0·430	2475	728	0·902
0·1459	0·526	1374	712	0·883
0·2441	0·625	835·4	701	0·868
0·4268	0·753	484·3	691	0·857
0·6379	0·861	325·5	688	0·853
0·8924	0·963	232·3	689	0·854
1·0759	1·025	191·9	692	0·857

Potassium Chloride II. KCl = 74·59.

m.	$m^{\frac{1}{3}}$.	R.	$k/m = \mu$.	μ/μ_∞.
0·02394	0·288	7894	754	0·934
0·04275	0·350	4504	741	0·919
0·07971	0·430	2475	724	0·897
0·1475	0·528	1374	705	0·873
0·2486	0·629	835·4	688	0·852
0·4408	0·761	484·3	669	0·829
0·6697	0·875	325·5	655	0·812
0·9575	0·986	232·3	642	0·796
1·1697	1·054	191·9	636	0·789

Barium Chloride. $\frac{1}{2}BaCl_2$ = 104·1.

In Cell No. 2, the water used had a resistance of 8·5 × 10^6 ohms.

m.	$m^{\frac{1}{3}}$.	R.	$k/m = \mu$.	μ/μ_∞.
0·02040	0·273	10856	644	0·863
0·05112	0·371	4584	609	0·816
0·1137	0·485	2178	577	0·773
0·2377	0·619	1095	549	0·735
0·4705	0·778	580·4	523	0·700
0·8710	0·955	325·5	504	0·674
1·741	1·203	172·2	476	0·638

Copper Sulphate. $\frac{1}{2}CuSO_4.5H_2O$ = 124·87.

Cell No. 2. The water had a resistance of 6·6 × 10^6 ohms.

m.	$m^{\frac{1}{3}}$.	R.	$k/m = \mu$.	μ/μ_∞.
0·02174	0·279	17173	382	0·548
0·07607	0·423	6397	293	0·421
0·2681	0·645	2359	226	0·324
0·5608	0·825	1313	193·9	0·279
0·8440	0·945	951·5	161·1	0·231
1·954	1·250	535·7	136·4	0·196

Potassium Bichromate. $\frac{1}{2}K_2Cr_2O_7 = 147\cdot3$.

In Cell No. 1, the water used had a resistance of $7\cdot26 \times 10^5$ ohms.

m.	$m^{\frac{1}{3}}$.	R.	$k/m = \mu$.	μ/μ_∞.
0·002278	0·132	3004	710	0·865
0·006047	0·182	1138	706	0·860
0·01475	0·245	472·2	698	0·850
0·04019	0·343	5240*	678	0·826
0·1010	0·466	2202*	642	0·782
0·2577	0·636	917·1*	604	0·736

Magnesium Sulphate. $\frac{1}{2}MgSO_4.7H_2O = 123\cdot26$.

In Cell No. 1, the water used had a resistance of $9\cdot75 \times 10^5$ ohms.

m.	$m^{\frac{1}{3}}$.	R.	$k/m = \mu$.	μ/μ_∞.
0·00000644	0·0186	511000	702	0·987
0·00001398	0·0241	330000	697	0·980
0·00003218	0·0318	179200	687	0·965
0·00007707	0·0426	84710	680	0·955
0·0003438	0·0701	21230	652	0·916

with, we cannot assume that it really gives the fraction of the number of the molecules which are at any moment ionized; in the light of probable changes in the ionic fluidity of the liquids, and of the possible existence of complex ions, such an assumption is clearly unjustified. For the sake of convenience, the results previously obtained, as well as those of the experiments now described, are collected in the following table:—

Equivalent Conductivities at 0° referred to the Limiting Value as Unity.

m = Number of Gram-equivalents of Solute per thousand grams of Solution.

m.	$m^{\frac{1}{3}}$.	KCl.	$\frac{1}{2}BaCl_2$.	$\frac{1}{2}K_2Cr_2O_7$.	$\frac{1}{2}CuSO_4$.	$\frac{1}{2}MgSO_4$.
0·00001	0·0215	1·000	1·000	0·991	0·998	0·983
0·00002	0·0272	1·000	1·000	0·930	0·993	0·976
0·00005	0·0368	1·000	0·998	0·952	0·981	0·963
0·0001	0·0464	0·999	0·995	0·929	0·967	0·950
0·0002	0·0585	0·998	9·990	0·902	0·947	0·932
0. 0005	0·0794	0·996	0·980	0·880	0·908	0·899
0·001	0·100	0·992	0·969	0·870	0·863	0·864
0·002	0·126	0·987	0·953	0·864	0·807	0·814
0·005	0·171	0·976	0·925	0·863	0·717	0·720
0·01	0·215	0·962	0·896	0·858	0·638	0·659
0·02	0·271	0·944	0·864	0·847	0·557	0·587
0·03	0·311	0·932	0·843	0·834	0·512	0·545
0·05	0·368	0·917	0·813	0·815	0·468	0·497
0·10	0·464	0·896	0·778	0·783	0·405	0·435
0·20	0·585	0·874	0·742	0·749	0·348	0·380
0·40	0·737	0·858	0·710	—	0·294	0·322
0·50	0·794	0·855	0·699	—	0·275	0·313
1·0	1·000	0·856	0·665	—	2·230	0·264
1·2	1·063	0·860	0·657	—	0·218	0·248
1·5	1·145	—	0·645	—	0·208	0·227
2·0	1·260	—	0·632	—	0·194	0·192
3·0	1·442	—	—	—	—	0·133

In the earlier set of experiments, approximate values only were obtained for the absolute equivalent conductivities, changes in the adjustment of the platinum cell between the experiments on each salt causing a slight uncertainty in the cell constant. From the values of the constants of the glass cells now used, it is possible to calculate throughout the whole range of concentration of both sets of observations the exact equivalent conductivities of the salts investigated. The results are given below, and are expressed in Kohlrausch's units, in which the electrical conductivities, measured in reciprocal ohms, are divided by the concentrations of the solutions, measured in gram-equivalents per cubic centimetre.

Equivalent Conductivities at 0° in Absolute Units.

m = number of Gram-equivalents of Solute per thousand grams of Solution.

m.	$m^{\frac{1}{3}}$.	KCl.	$\frac{1}{2}BaCl_2$.	$\frac{1}{2}K_2Cr_3O_7$.	$\frac{1}{2}CuSO_4$.	$\frac{1}{2}MgSO_4$.
0·00001	0·0215	807	746	813	696	699
0·00002	0·0272	807	746	804	692	694
0·00005	0·0368	807	745	781	684	685
0·0001	0·0464	806	742	763	674	676
0·0002	0·0585	806	739	740	660	663
0·0005	0·0794	803	731	722	633	639
0·001	0·100	800	723	714	602	614
0·002	0·126	796	711	709	563	579
0·005	0·171	787	690	708	500	512
0·01	0·215	776	669	704	445	468
0·02	0·271	761	645	695	388	417
0·03	0·311	752	629	685	357	387
0·05	0·368	740	607	669	326	353
0·10	0·464	723	581	643	282	309
0·20	0·585	705	554	615	243	270
0·40	0·737	692	530	—	205	229
0·50	0·794	690	522	—	192	222
1·0	1·000	690	496	—	160	188
1·2	1·063	694	490	—	152	176
1·5	1·145	—	481	—	145	161
2·0	1·260	—	472	—	135	136
3·0	1·442	—	—	—	—	94·5

I beg the honour of laying before the Royal Society the following more complete consideration of the subject.

The above-mentioned article had for its object the study, from careful measurements made at my request by Mr. Déguisne,* of the influence of temperature on completely dissociated, that is, infinitely dilute aqueous solutions of strong electrolytes, and the deduction from this of the temperature coefficients of the single ions.

Mr. Déguisne expresses the influence of temperature on the conductivity κ, starting from 18° as a mean temperature, in the form of the quadratic interpolations equation

$$\kappa_t = \kappa_{18}[1+\alpha(t-18)+\beta(t-18)^2]$$

and shows that this closely represents his observations between 2° and 34° C. I shall confine myself to the consideration of this formula.

(1.) *Numerical values.*—In the table, under Λ is given the equivalent conductivity of infinitely dilute solutions,† then the coefficients α and β for 1/1000 and, for the neutral salts, observed by Déguisne, α also for 1/10000 normal solutions, corrected by me for the impurity of the water. Mr. Déguisne has kindly furnished me with the data regarding the water in each of his solutions. After applying these corrections the values for 1/10000 and 1/1000 normal do not continue to show the systematic changes, which from the observations appeared to

	Λ.	α for		β.
		1/10000.	1/1000.	
KCl	130·1	0·0216	0·0217	+0·000066
KF	111·3	..	226	069
KNO_3	126·4	210	211	062
K_2SO_4	133·4	223	223	077
KOH	239	..	190	033
NH_4Cl	130	219	219	068
NaCl	109·0	228	227	084
NaF	90·2	..	240	100
$NaNO_3$	105·3	220	221	075
Na_2SO_4	112·2	234	234	097
$NaC_2H_3O_2$	78·5	..	242	110
$NaC_5H_9O_2$	69·2	..	244	111
LiCl	98·9	..	233	091
$BaCl_2$	121·7	227	226	083
$Ba(NO_3)_2$	118·1	221	220	076
$AgNO_3$	115·8	216	216	+0·000067
HCl	383	..	165	−0·000015
HNO_3	380	..	163	−0·000017

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* Déguisne, "Temperatur-Koefficienten des Leitvermögens sehr verdünnter wässriger Lösungen," 'Dissertation, Strassburg,' 1893.

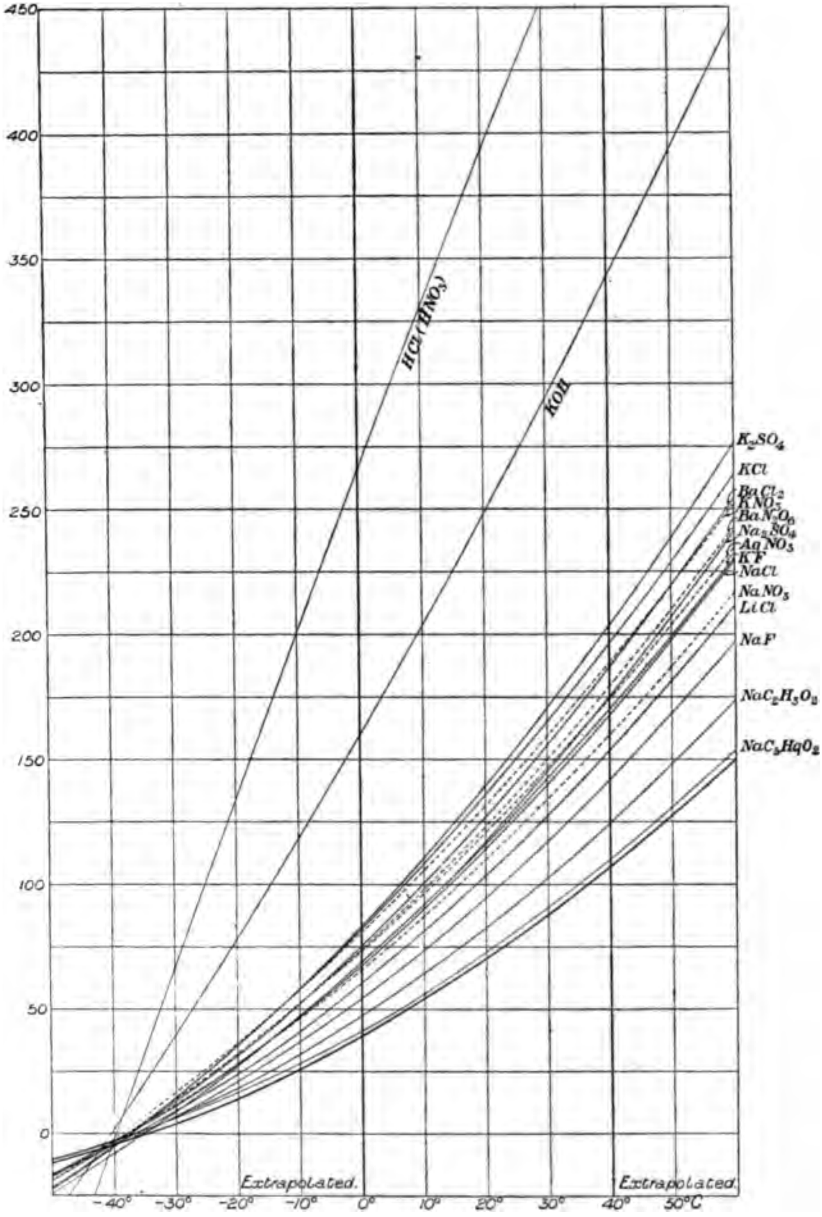
† Kohlrausch u. v. Steinwehr. 'Sitz. Ber. Berlin, Akad.,' 1902, p. 581.

make the reduction to completely dissociated solutions very uncertain, but now show only differences of irregular sign within the limits of the errors of observation.

From this we may assume that the true values of the coefficients for the concentration 1/10000 would be practically the same as those for complete dissociation. But, as a matter of fact, in the case of such dilute solutions considerable uncertainty underlies the observations themselves, on account of the variations of the solutions with time, as well as the corrections for the conductivity of the water, on account of its magnitude and somewhat uncertain theoretical basis. These facts are especially true in the case of the acids. Therefore, the values found for the concentration 1/1000, in which these two sources of error need scarcely be considered, must be looked upon as experimentally much more accurate. Then too, the condition of ionization of very dilute solutions of weak salts is always somewhat uncertain on account of possible hydrolytic action. In order to treat all electrolytes alike, I shall therefore choose the coefficients α and β of the concentration 1/1000.

The errors which may arise from the fact that these solutions lack from 2 to 5 per cent. of complete dissociation cannot at present be avoided. Since the values of the constants themselves differ but little from those for a concentration of 1/10000, we may assume that

FIG. 1.



In regard to the lowest curve, *cf.* 4. The fact that the greatest differences exist for KOH and KF may be connected with the smaller accuracy of these curves. But on this we will lay no stress.

(3.) *Discussion of Results.*—We may derive from the above fact the ertainty that the individual differences of the electrolytes come under common law, the degree of accuracy of which must, however, remain nsettled. This law I have stated as follows: for dissociated aqueous olutions the coefficient β of the quadratic member can be approximately expressed in terms of the coefficient α of the linear member, in he form $\beta = C(\alpha - A)$, where C and A are constants common to all lectrolytes. One sees at once that this law is identical with the ther; all curves of the expression $(1 + \alpha t + \beta t^2)$ pass through the same oint, having for its abscissa $-1/C$.*

The proposed constants have the following values, taking 18° C. as he point from which the temperature is reckoned, $C = 0{\cdot}0163$, $A = 0{\cdot}0174$. The convergence takes place at the point where $-18 = -1/0{\cdot}0163 = -61$, or $t = -43°$. On account of the small ifference between C and A, this point lies not far from the zero axis. f C and A were identical, the extrapolation according to the quadratic ormula would show that the conductivity of all electrolytes becomes ero at the same temperature. Introducing this critical temperature t_0, ll electrolytes could be nearly represented by a formula containing but wo individual constants,

$$\kappa_t = P(t - t_0) + Q(t - t_0)^2.$$

at moderate temperatures with the quadratic interpolation formula, already applied by Poiseuille, has long been known to me. The literary priority regarding this observation belongs, however, to Messrs. Bousfield and Lowry.

In my calculations I have made use of the values chosen by Mr. Heydweiller,* as the most probable between the limits of 0° and 30°. These values were selected from different observers, especially Messrs. Thorpe and Rodger. Since the constants were calculated for this range of temperature, they have practically the same signification for the fluidity of water as the former constants for the conductivity.

The formula

$$\phi_t = 55 \cdot 68 + 1 \cdot 981t + 0 \cdot 0105t^2 \text{ C.G.S.}$$

or

$$\phi_t = 94 \cdot 74[1 + 0 \cdot 0249(t-18) + 0 \cdot 000111(t-18)^2] \text{ C.G.S.}$$

represents the fluidity ϕ within the limits of the table, 0° to 30°, with a maximum error of 1/1000, that is, with about the same degree of accuracy as Déguisne's formula for the conductivity. As far as 90° the error would not exceed 1/100.

The curve represented in the figure instead of having the factor 94·74 (which has no reference to the conductivity), was given the arbitrary factor 67·0 in order to give the curve the desired position close to the lowest curve of conductivity, that of sodium valerate.

The coincidence of the two curves is striking, indeed the coefficients 0·0249 and 0·000111 differ little from those of sodium valerate 0·0244 and 0·000111.† The curves of conductivity are cut by the fluidity curve approximately in the same region to which they converge. The curve of fluidity passes through zero at − 34° C.‡

Messrs. Bousfield and Lowry calculated from the measurements of Thorpe and Rodger the coefficients 0·0251 and 0·000115. This curve differs very little from mine, especially in the neighbourhood of the crossing point.

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* Mr. Heydweiller calculated at my request the table for the 'Lehrbuch der Praktischen Physik' (Tab. 20a, 1901).

† The same would apply for Déguisne's observations on Na_2HPO_4 (0·0241 and 0·000105) and on $NaHC_4H_4O_4$ (0·0241 and 0·000109), which, however, on account of the unknown constitution of these salts in solution, I have left out of account.

Messrs. Bousfield and Lowry further called attention to the fact that the temperature change which I have found for ordinary distilled water (practically a very dilute solution of CO_2) corresponds with the temperature change of fluidity.

‡ It is also to be mentioned that Messrs. Lyle and Hosking, from their interesting observations on the viscosity and the electrical resistance of 0·1 to 4 normal solutions of NaCl between 0° and 100° draw the conclusion: "The curves so arrived at are remarkable, in that they indicate that for solutions of the strengths experimented with, both the fluidity and the sp. mol. conductivity vanish at a temperature of −35°·5 C." The manner of extrapolation is not dealt with.—'Phil. Mag.,' May, 1902, p. 496.

The fact is therefore established that the temperature change of the fluidity of water is nearly the same as that of the conductivity of dissociated aqueous solutions of electrolytes which have a large temperature coefficient. Even if nothing more was known than this fact, the question of a connection between the electrolytic and the mechanical motion in water must be considered a matter for serious discussion.

(5.) *Discussion of the Extrapolation.*—Extrapolation of an empirical ormula over a wide range can never be considered as necessarily representing the truth. This is especially true in a case like the present, where at low temperatures it is applied to a state of matter other than hat for which the formula was originally deduced. It is *à priori* impossible for the formula to hold where its extrapolation gives to the conductivity or the fluidity a value zero. Since these quantities are from heir very nature positive, negative values are physically impossible. Therefore the cutting of the zero axis by the curve at an acute angle is *à priori* inadmissible, just as, for example, the assumption is inadmissible that the Joule heating effect is proportional to the current trength, or that the kinetic energy is proportional to the velocity. A quantity from its nature positive can, as it becomes zero, have a nite differential quotient as function of another quantity, only when he other quantity cannot vary beyond the critical point. This can

(6.) *Other Formulæ.*—Among the former attempts to derive an empirical formula from the behaviour in ordinary temperatures, which does not *à priori* lose its significance as the conductivity or the fluidity approaches zero, that of Slotte, $\phi = \phi_0 (1 + bt)^n$, must be given the first place. This has recently assumed a greater importance on account of the extensive work of Thorpe and Rodger. Here, in the case of the fluidity, the condition that $d\phi/dt = 0$ where $\phi = 0$ is fulfilled, for here everywhere $n > 1$. For the conductivity, however, nothing is gained, since in the case of the acids $n < 1$, and there $d\kappa/dt = \infty$ for $\kappa = 0$; there is, therefore, no object in recalculating our results according to this formula. However, in a former paper I have given the preference to Slotte's formula in a remark concerning fluidity. To try to bring the coefficients of this formula into a relationship with those of the quadratic formula was indeed not allowable.

(7.) *Experimental Indications.*—The attempt to draw conclusions in regard to the region near the zero value from the course of the phenomenon in the region where the fluidity &c. have values of considerable magnitude, would have, of course, very little prospect of success. But perhaps the attempt to follow the fluidity of water or the conductivity of dilute solutions down to lower temperatures would be more successful than we think, if made in closed vessels of small dimensions.

Up to the present time we are acquainted only with phenomena from which uncertain conclusions from analogy can be drawn. The idea may be pretty certainly held as probable, that the viscosity and the electrical resistance finally increase more slowly than the quadratic formula extrapolated would indicate. In regard to viscosity, I remember the investigations of Tammann on the freezing of overcooled liquids, and Ostwald's observations on salol. The phenomenon of the gradual solidification of alcohol at low temperatures also leads to the same conclusion.

Numerous observations of the same kind in regard to electrical behaviour are also recorded. The very slow increase of the conductivity of glass with increasing temperature is well known. A quantitative determination of this has been made by Messrs. Bousfield and Lowry. But how far a heterogeneous mixture like glass can be considered parallel to a dilute ionised solution is doubtful. In the same way, experiments such as those of Lehmann on the electrolysis

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introduction of errors is without much weight; it is, at any rate, practically the same case as that of the gas formula. This is written, making use of the absolute zero, $vp = \text{const.}\ (t_0 + t)$, where $t_0 = 273$, although it is certain that the result $v = 0$ for $t = -t_0$ is false, so that the formula must assume another form before v approaches its zero value. The constant of the gas formula loses its significance at the point where the gas passes into the liquid state. The same is true for the temperature constant of the fluidity, which we have introduced, when it passes out of the liquid state.

f solid silver iodide, or those of W. Kohlrausch on its conductivity at ifferent temperatures, scarcely form a basis for conclusions in regard o solutions in water, particularly as the behaviour of silver chloride nd silver bromide is quite different from that of silver iodide.

In addition, it is difficult to compare substances which are so highly oncentrated, in comparison with ordinary electrolytes, with dilute olutions. All these conductivities reduced in the ratio of the con-entration, as, for example, the measurements made on hot glass of arge surface and small thickness by means of the potentiometer, would robably have to be considered zero, that is, smaller than the errors f observation, in comparison with our dilute salt solutions at moderate emperatures.

Of the greatest interest for our problem are the recently published neasurements of electral conductivity as far as − 70° C., which Mr. unz* was led to make through my remarks on the relation between onductivity and temperature. He was unfortunately unable (as was also) to sufficiently over-cool dilute solutions, and the measurements vere therefore made on strong solutions of sulphuric acid of at least gr. equiv./litre. Notwithstanding the fact that no certain conclu-ions can be drawn in regard to dilute solutions, Mr. Kunz's values re of sufficient interest to represent graphically. The curves are narked with the percentage concentration of the solutions (fig. 2).

conductivity, so that it may be supposed that the 19 per cent. solution would reach a relatively negligible value at a much higher temperature than the stronger solutions, if it were possible to follow it.

Mr. Kunz adopts the view that the electrical resistance is due to friction. It is his opinion, indeed, that the conductivity would disappear only at the absolute zero of temperature; this conclusion can hardly be supported by his observations, as his lowest temperatures are still +200° absolute.

(8.) *The Temperature Coefficients of the Single Ions.*—For these* I have recently published values. It is of importance to us that the temperature coefficients of univalent monatomic ions appear to be functions of mobility, decreasing as the mobility increases. Complex and multivalent ions as groups deviate from this series, so that in this relationship we have a new criterion for univalent elemental ions. The largest temperature coefficients of the ions approach that of water.

(9.) *The Electrolytic Resistance Considered as Friction of the Solvent.*—In the common view concerning the motion of the ions, an assumption is tacitly made which in other cases we do not consider justifiable. In the relative motions of adjacent particles we assume a discontinuity only in the case of friction between two rigid bodies where this by definition must occur. Even here it is impossible to deny that on the actual surfaces of contact there may be connected with the motion a rubbing away of particles which produces a continuous variation of velocity from one to the other.

When a fluid is in question, whether in contact with another fluid or with a solid, we concede no finite variation of velocity in two points at an infinitesimal distance from each other. The primitive assumption, until recently held to be correct, in the case of mercury on glass, that the fluid in actual contact with the solid moved with a finite velocity, would demand that the external friction be infinitesimal in comparison with the internal. This is now, to the best of my knowledge, entirely given up.† The idea of discontinuity, however, we employ in regard to the ions when we think of them as moving through the solvent without connection with it.

In addition to the objection of discontinuity there exist also the following difficulties in this assumption. In the first place, it is difficult to see how the electrical energy passes into the solvent in the form of heat, unless the latter takes part in the motion of translation. Further, it seems probable, from the fact of the ionisation of the salts, that forces must exist between the ions and the water. Ciamician,* twelve years ago, concluded that this must lead to the hypothesis of a water-shell about the ion. The attempt was also made to measure this hydration, by using a method proposed by Nernst.

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* 'Sitz. Ber. d. Berlin. Akademie,' 1902, p. 572. The values here given are strengthened by the fact that a linear connection between α and β (*cf.* 3) appears *à priori* in the case of the single ions as well as in the case of electrolytes.

† Comp. Warburg, 'Pogg. Ann.,' vol. 140, 1870, p. 379. The "slipping" of rarefied gases on solid surfaces, established by Kundt and Warburg, being a separate phenomenon, need not be considered here.

We may, therefore, look upon it as probable that the moving ion carries with it a mass of adhering solvent, just as a moving immersed solid carries with it a portion of the liquid, and we will endeavour on this basis to construct a new representation of electrolytic resistance. According to this the mechanics of electrolysis assume an appearance quite different from the old hypothesis of isolated ions. The resistance to motion takes place not directly between the solvent and the ions, not between H_2O and K or Cl, &c., but it is a phenomenon of friction between the particles of the solvent itself, modified by the fact that the accompanying shell of solvent may be thin enough to allow the ion to act through it, to a certain extent, upon the outer liquid.

It is impossible at first to make anything more than this rough sketch of the hypothesis, and the more so, as the expressions "continuous" and "discontinuous" must be especially defined if we are to apply them to molecular processes. The expression "continuous" stands from its very nature in contradiction to the atomic or molecular hypothesis; and in the case of solutions, and especially in electrolysis, a molecular representation seems to be the only one which is scientific-between the ion and the outer portion of the solvent diminishes as the atmosphere becomes of greater thickness.

Conclusions.—(*a.*) The electrical resistance of an ion, expressed in mechanical units, must be of the same order of magnitude as the mechanical frictional resistance of a molecule of the solvent; a law whose assumption, as I some time ago showed, "leads to an expression for the distance between the molecules which is comparable with the usually accepted views in regard to this quantity."*

(*b.*) The empirically discovered law that the temperature change of the resistance of the most sluggish ions is very like the temperature change of the viscosity of water, becomes now understandable. For ions of large resistance we must assume that the atmosphere is of considerable thickness, and hence the action of the ion itself on the outer portion of the solvent will be small. As a limiting case, for a very sluggish ion there will be only the friction of water against water, and the electrolytic resistance will have the same temperature coefficient as the viscosity of water, provided that the atmosphere itself does not change its dimensions with the temperature. If, however, the atmosphere become, for example, smaller with increasing temperature, the temperature gradient of the conductivity might be greater than that of the fluidity. According to the observations now at hand, this would seem to be the case for the slowest moving univalent ion Li. Even here, however, the differences scarcely exceed the errors of observation.

(*c.*) I now come to the remarkable relationship between the mobility of the ions and their temperature coefficients, which was mentioned in Section 8. This first led me to seek a general explanation for the electrolytic resistance in the idea of a water atmosphere, in order to escape being compelled to explain this otherwise unreconcilable fundamental characteristic of the ions as a *deus ex machina.*

Assuming as the single fundamental characteristic of each univalent monatomic ion the formation of a water atmosphere which varies according to the nature of the ion, the mobility of this complex on the one side, and its temperature coefficient on the other, will be functions of these atmospheric formations, and therefore both quantities must hold functional relations to each other. We know too little of the molecular forces at present to attempt to describe this connection more exactly. But for the case in which the water shell is so thick that the ion exerts no force beyond it, the resistance to motion becomes simply a matter of water friction, which explains the fact that the most sluggish ions have nearly the same temperature coefficients as the viscosity. In the case of smaller aggregations, we must remain content with the fact that we have at least the possibility of a fundamental explanation.

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* 'Göttinger Nachrichten,' 1879, p. 1.

There are two experimental questions which are of importance lthough difficult to answer: first, whether the functional relationship is xact or only approximate, and second, whether the positive and egative ions are fully identical in regard to this relationship.

That the non-elementary ions also show as their greatest temperature oefficients that of water friction, but that they as groups differ from he elementary ions, is to be expected. The latter fact cannot be uantitatively explained. Here also it will be necessary to wait for ore exact experimental data to settle the question.

(*d.*) Finally, the indication of the temperature formula that the obility of all the ions converges towards zero (*cf.* 2 and 3) at about he same temperature, is a logical result, if the electrolytic resistance s in reality a mechanical friction. The fact that the formula for the uidity of water takes part in this convergence, gives the hypothesis urther support.

It does not seem at all impossible that the deviations from a strictly ommon zero point, found in extrapolating the different formulæ, have systematic cause. These divergences seem to indicate that the ore mobility an ion shows at ordinary temperatures, the more slowly elatively it loses the residue of its mobility as the solvent becomes ore viscous. The mobility of the water molecules themselves ecomes small at a comparatively high temperature, where such ions

"Upon the Immunising Effects of the Intracellular Contents of the Typhoid Bacillus as obtained by the Disintegration of the Organism at the Temperature of Liquid Air." By ALLAN MACFADYEN, M.D. Communicated by LORD LISTER, O.M., F.R.S.

Received February 19,—Read March 12, 1903.

In a previous communication* it was shown that it was possible to disintegrate mechanically the typhoid bacillus at the temperature of liquid air, and to obtain the cell-juices of the organism.

The typhoid cell-juices obtained by this method on inoculation into animals proved toxic or fatal. It was, therefore, concluded that the typhoid bacillus contained within itself an intracellular toxin.

It remained to test the typhoid cell-juices for immuninising and other properties. The preliminary experiments in this direction, which form the subject of the present note, were made upon the monkey. The monkey was selected as an animal most likely to furnish data of possible application to man. For this purpose the typhoid cell-juice was administered subcutaneously to the monkey. The injections did not produce any general symptoms beyond a transient rise in temperature, whilst the material was quickly absorbed after each injection without any traceable local effect. In this manner doses of 0·5 to 1 c.c. of the material were injected at intervals. An immediate result was the agglutination of the typhoid bacillus by the serum of the blood of the treated monkeys, whereas no such effect was produced by the serum of monkeys which had not been treated. This furnished useful evidence that the animals were under the influence of cell-juices derived from the typhoid organism. The injections were repeated at intervals of three to four days, and after a lapse of four to six weeks the animals were bled. The serum obtained was then tested for immunising properties. The test objects were (1) a virulent culture of the typhoid bacillus, and (2) the intracellular toxic juice of the same organism. A varying amount of the virulent bacilli and of their toxic cell-juice was mixed with a varying quantity of the serum. The respective mixtures were then injected into the peritoneal cavity of the guinea-pig.

The broth cultures of the typhoid organism used in the experiments were *per se* lethal in doses of 0·1 c.c. in five to ten hours. The typhoid cell-juices were fatal in doses of 0·2 and 0·1 c.c. in three to five hours, and in doses of 0·05 c.c. in about twelve hours. The serum was thus tested for (1) specific antibacterial and (2) specific antitoxic properties.

The experiments showed that the serum of the monkey, after injections of the typhoid cell-juices, possessed antibacterial and antitoxic roperties, inasmuch as the serum protected the experimental animals gainst the bacilli, and also against an intracellular toxin obtained rom them.

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* 'Roy. Soc. Proc.,' *supra*, p. 76.

A simultaneous injection of (1) serum with the bacilli, and (2) serum ith the toxic cell-juice produced no lethal or toxic effects. The ontrol animals, on the other hand, invariably succumbed.

It was further investigated whether the serum possessed preventive nd curative properties. The serum from treated monkeys was injected to guinea-pigs, one injection being made in each instance, and the me animals received at an interval of 12 to 24 hours lethal doses of he typhoid bacillus and of its toxic intracellular juice respectively. he treated animals survived the test, whilst the control animals uccumbed. It was therefore concluded that the serum had protective roperties.

A third series of guinea-pigs received lethal doses of the typhoid acillus and of its toxic cell-juice respectively. The serum was then jected at various intervals into individual animals. It was found hat the lives of the animals could be saved by one injection of the serum rom a fatal infection or intoxication, even when half of the lethal period ad elapsed in each instance. The serum, therefore, possessed curative roperties.

In view of the above results it appeared desirable to test the effect

“On the Histology of *Uredo dispersa*, Erikss., and the ‘Mycoplasm’ Hypothesis.”

By H. MARSHALL WARD, Sc.D., F.R.S., Professor of Botany in the University of Cambridge. Received February 13,—Read March 12, 1903.

(Abstract.)

The paper deals with a detailed study of the histological features of the germination, infection, and growth of the mycelium of the Uredo in the tissue of grasses. Primarily, the figures refer especially to the *Uredo* of *Puccinia dispersa* in the tissues of *Bromus secalinus*, but comparisons are made with the behaviour of this and other Uredineæ—*e.g.*, *Puccinia glumarum* and *P. graminis*—in the tissues of other grasses and cereals.

The research, which has been carried on over a year and a half and has involved the preparation and microscopic examination of thousands of sections, is principally based on the application of improved hardening and staining methods to preparations from tube cultures of the grasses concerned, the leaves of which were infected at definite spots. These tube-cultures were prepared according to the method previously described.* At definite intervals after sowing the spores—*e.g.*, after 1, 2, to 6 and 8 days—the infected areas were removed and placed in fixing solutions, and the life-history of the fungus traced step by step, and controlled by reference to uninfected areas.

The full paper is illustrated by numerous figures, and deals with the behaviour of the nuclei, vacuoles, septa, branches, haustoria, and other details of the hyphæ up to the commencement of spore-formation.

The relations of the hyphæ and haustoria to the cell-contents of the host are critically examined, and the cumulative evidence not only fails to support Eriksson’s *Mycoplasm* hypothesis; but is completely subversive of it, so far as histological facts are concerned.

Eriksson’s hypothesis, which refers the epidemic outbreaks of rust to the sudden transformation into the mycelial form of a supposed infective substance, previously latent and invisible in the cytoplasm of the host, is shown to be untenable because the *corpuscules spéciaux* of this author are proved to be the cut-off haustoria of the fungus.

Eriksson supposes that these *corpuscules* (haustoria) are formed by the hitherto latent germs in the host-cells, growing up in the cells into vesicles, which then pierce the cell-walls and give rise to hyphæ in the intercellular spaces.

The present paper shows that Eriksson has entirely reversed the true order of events. The haustoria have been formed by the hyphæ, and figures are given showing every stage in their development. The first haustorium may be formed by the infecting tube immediately after its penetration through the stoma, and figures are given showing the remains of the germ-tube outside a stoma, the swelling of its tip over the stoma into an appressorium, the passage through the stomatal cavity, and its development into a vesicular swelling whence the true infection tube arises, which latter may at once put forth a haustorium. In some cases all these latter phenomena are visible in one and the same preparation.

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* “On Pure Cultures of a Uredine, *Puccinia dispersa* (Erikss.)” ‘Roy. Soc. Proc.,’ 1902, vol. 69, p. 461.

The author expresses his thanks to Miss E. Dale, of Girton College, for valuable aid during the later stages of the work, in the embedding and cutting of numerous sections.

"The Œstrous Cycle and the Formation of the Corpus Luteum in the Sheep." By Francis H. A. Marshall, B.A. Communicated by Professor J. C. Ewart, F.R.S. Received February 17,—Read March 12, 1903.

occur. The extravasated blood retained in the mucosa forms pigment, the change being effected by the agency of leucocytes, as supposed by Bonnet, but not by Kazzander. But the extravasation occurs in the superficial part of the mucosa. Miniature lacunæ are sometimes formed after extravasation. The severity of the procestrous process tends to diminish with each successive diœstrous cycle in a season. There is never any removal of stroma and not necessarily of epithelium.

The facts recorded render the homology between the diœstrous cycle in the sheep and the menstrual cycle of the Primates very probable, while further statements regarding the author's researches on the ferret, with which the proœstrum in regard to severity is intermediate between that of the sheep and menstruation in monkeys, afford additional evidence of the identity of the two processes.

Ovulation, with Notes on the Atretic Follicle and the Causes of Barrenness.—Ovulation can occur spontaneously at any œstrous (or proœstrous) period with Scotch black-faced sheep, excepting at certain œstri outside the regular sexual season, when the additional stimulation supplied by coition may be necessary. In the ferret, ovulation does not occur in the absence of coition, without which the follicles undergo atresia. In the sheep, atresia is commonest in follicles of about one-eighth to one-half the dimensions of the mature follicles. When it occurs with any considerable frequency, it must affect the barrenness percentage in subsequent breeding seasons. The atretic follicle differs from the developing corpus luteum in the absence of any discharge to the exterior, the membrana granulosa degenerating and disappearing prior to any considerable ingrowth from the connective tissue wall.

The Formation of the Corpus Luteum.—The lutein cells are derived from the membrana granulosa, while the connective tissue element is supplied by the proliferation and ingrowth of the thecæ interna and externa, as described in the preliminary communication. Leucocytes are abundant, especially at the sixteen-hour stage of development, but these disappear in later stages without giving rise to connective tissue as described by Sobotta. The cavity of the discharged follicle is filled in by the further ingrowth of connective tissue.

On the Culture of the Nitroso-bacterium." By H. S. FREMLIN, Lymph Laboratories, Chelsea Bridge. Communicated by Sir MICHAEL FOSTER, K.C.B., Sec. R.S. Received February 23,—Read March 12, 1903.

Carried on at Westminster Hospital Medical School and the Jenner Institute of Preventive Medicine.)

My object in undertaking this work was in the first place to obtain pure culture of the nitroso-bacterium; in the second place to discover hether it really was a fact that this species was unable to grow in e presence of organic matter, as stated by Winogradsky.*

My experiments with the nitroso-bacterium appear to show that:—

1. A practically pure culture of the bacterium can be obtained ter sub-culturing for 7 months in Winogradsky's ammonia solution.

2. That the nitroso-bacterium will grow in this solution in the esence of organic matter.

3. That the nitroso-bacterium will grow not only on silica jelly, but so in any ordinary organic medium.

In the course of these experiments pure cultures were again and always been tested for the presence of oxides of nitrogen, and a control set up when batches of the solution were inoculated.

The presence of nitrites was judged by a solution of diphenylamine in sulphuric acid, Ilosvay's solution being used as a control when necessary.

When I commenced work I obtained cultures of the nitroso-bacterium by inoculating ammonia solutions with small quantities, 0·2 gramme or less, of various kinds of soil; rich garden soil, humus, sand, &c. Tubes of ammonia solution so inoculated were kept at room temperature and placed in a dark cupboard in order to avoid exposure to light.

The evidence of the growth of the nitroso-bacteria was found in the conversion of the ammonia in the solution into nitrous acid. This is at first a slow process and does not commence in the tubes for some 3 weeks as a rule; usually it requires another week or two to be completed.

I inoculated ammonia tubes with soil 43 times. Of these 70 per cent. showed oxidation of the ammonia.

Gelatine plates poured from these tubes showed moulds, yeasts, liquefying and non-liquefying bacteria, and also a micro-organism morphologically similar to the nitroso-bacterium.

1*st Dilutions.*—From the ammonia tubes which showed oxidation sub-cultures were made in like media. Of these 77 per cent. showed nitrites. This occurred in 8 weeks.

2*nd Dilutions.*—From the first dilutions sub-cultures were made. Of these 85 per cent. showed oxidation in from 2 to 3 months.

3*rd Dilutions.*—From the second dilutions sub-cultures were made. All these showed formation of nitrite in one month.

4*th Dilutions.*—Sub-cultures from the third dilutions exhibited formation of nitrite in 93 per cent. of the tubes inoculated. These, as a rule, required 6 weeks before this was completed.

5*th Dilutions.*—Sub-cultures were made from the fourth dilution tubes. Of these 77 per cent. showed the formation of nitrite; the time required being 2 months.

With regard to the microscopical specimens made from the several tubes; the 1st, 2nd, and 3rd dilutions showed, associated with the nitroso-bacterium, rod-shaped micro-organisms, in gradually diminishing numbers in succeeding dilutions. The 4th and 5th dilutions gave almost pure culture of the nitroso-bacterium apparently.

Ammonia Solution containing no Carbonate.

A medium containing simply ammonium sulphate and potassium phosphate was tried.

The nitroso-bacterium was able to grow in this, and to produce *nitrite; but the* micro-organism did not develop in sub-cultures.

Liquid Media containing Organic Matter.

I made a series of experiments with "ammonia solutions" containng peptone beef broth, Witte's powdered peptone, and urea.

Beef Broth.

This was added in quantities varying from 1 in 11000 to 10 in 100.

Cultures of nitroso-bacteria grew well when inoculated from norganic solutions into the lower percentages of beef broth, and on ransferring them to higher percentages they were able to continue heir nitrification. If cultures of nitroso-bacteria were taken from norganic solutions and placed directly into solutions containing beef roth to the extent of 1 in 1000 they failed to show oxidation.

Peptone.

This was used in solutions containing 1 in 11000 and 1 in 5000.

The nitroso-bacterium was able to grow in these solutions; its levelopment being satisfactory, as shown by the formation of nitrite.

Urea.

Isolation of the Nitroso-bacterium.

Plate Cultures.

In carrying out the work of isolation of the nitroso-bacterium I made plate cultures containing silica, gelatine, and agar media respectively.

Silica Plates.

I found that numerous species of micro-organisms grew on this medium, and that, therefore, it was necessary to use what seemed to be pure cultures of the nitroso-bacterium, as this species does not form colonies rapidly, and is liable to be smothered by the more quickly-growing bacteria if one attempts to isolate the nitroso-bacterium from soil or a very impure culture. The nitroso-bacterium grows well in this medium, and in one instance I was able to remove a single colony which oxidised the ammonia in a solution. From this culture inoculation was made into beef-broth agar, and plates poured. These plates grew large numbers of colonies in pure cultures.

Numerous colonies were taken from silica plates and grown on beef-broth agar, but such growth transferred to ammonia solutions did not produce nitrification. This being the case I thought that probably the micro-organism had lost its power of oxidising the ammonia, so that I tried to devise a means by which this function might be re-established; and for this purpose the micro-organism was placed in as natural surroundings as were attainable. The following was the method adopted. A single colony was taken from silica plate and inoculated on to sloping beef-broth agar. After growing there it was transferred to a sterile ammonia solution; this was allowed to filter daily through sterile soil, thus allowing of aeration of the growth whilst in its natural surroundings. This experiment succeeded, nitrite being formed in 10 weeks. A control filter showed no change.

Gelatine Plates.

Winogradsky states that the nitroso-bacterium does not grow on gelatine; so that, in the first place, the method that he advocates to obtain a pure culture of nitroso-bacteria was adopted.

Particles of magnesia were removed from an oxidised ammonia solution and sown on to gelatine plates. Now if these particles carried nitroso-bacteria alone there would, if Winogradsky be correct, be no growth, and such a particle showing no growth could be removed and reinoculated into an ammonia solution, and thus a pure culture obtained. But I found that around particles so inoculated into gelatine, colonies invariably occurred. It was noted that these colonies were in pure culture and were made up of an oval organism that was morphologically similar to the nitroso-bacterium.

Secondly, gelatine plates were poured from oxidised ammonia soluions; these also gave the same species of micro-organism, often in ractically pure culture. From one such plate a piece was removed, nd in an ammonia solution it produced oxidation. This oxidised mmonia solution again yielded the same species on gelatine plates.

Gelatine media prepared from divers soils and inoculated with xidised ammonia solutions exhibited the same species of micro-rganism.

Hence it is to be inferred that, either the nitroso-bacterium grows n gelatine, or that an organism morphologically similar occurs in the ame inorganic solutions with the nitroso-bacterium and thrives like it n inorganic solutions.

To arrive at some definite conclusion on this point further experinents were made with agar plates.

Agar Plates.

In commencing my researches with Agar, beef-broth agar was used.

Beef-broth Agar.

Plates of this medium, inoculated from cultures containing nitroso-acteria gave similar results to those with gelatine; that is to say, The salts are dissolved, and agar added to 1½ per cent.; the whole being boiled up and prepared as ordinary agar. Magnesium carbonate is added after sterilisation.

As will be seen, this agar corresponds in composition to the ammonia solution used for the ordinary cultures, save for the presence of the 1½ per cent. agar. It has a slightly lower melting and coagulation point than bouillon agar.

I have poured over 100 plates of this medium. It grows the nitroso-bacterium well, oxidation of the ammonia in the plate occurring within 2 months, as a rule, after inoculation with oxidised ammonia solutions.

All plates that showed oxidation of the ammonia contained large numbers of colonies of apparently the same bacterium. This organism being oval in form, and associated with the formation of nitrite, and being often almost or altogether in pure culture, must be considered to be the nitroso-bacterium. It occurred in the dilution plates in all instances. Nevertheless but few of these showed oxidation of the ammonia.

The following table gives results obtained:—

	Plates, number poured.	Number showing oxidation.
Original..............	26	22
First dilution..........	26	3
Second dilution........	26	1

This shows that unless the colonies were numerous nitrite was not formed.

In one instance a single colony taken from an ammonia agar plate and placed on sloping ammonia agar formed nitrite in 9 months. Plates of beef-broth agar and gelatine poured from this culture grew enormous numbers of the colonies. These colonies develop both at room temperature and 37° C. At room temperature, after 6 days, the colonies appear to the naked eye as white iridescent growths varying in size. Some days later they become lemon coloured, and finally yellow. Under the microscope they were seen to be made of micro-organisms corresponding to the nitroso-bacteria.

The Statolith-theory of Geotropism." By FRANCIS DARWIN, M.A., M.B., F.R.S.

Received March 6,—Read March 12, 1903.

To make clear the point of view from which the experiments here corded were undertaken, a few words on the modern theory of avitational sensitiveness are necessary. For fuller details the ader is referred to the papers of Noll, Němec and Haberlandt,* to hom the theory in question is due.

When a geotropic organ suffers a change of position it responds by appropriate curvature, and ultimately reassumes verticality. With e mechanism of curvature I am not concerned, the problem to solved is the source of the stimulus to which the curvature is a sponse. In other words: when a plant, which normally grows vertilly upwards, is laid horizontally, it obviously perceives the change; what mechanism is the perception rendered possible?

The view of Němec and Haberlandt is that the stimulus is due to e presence of bodies of greater specific gravity than the cell sap, hich in consequence of their weight always fall to the physically wer regions of the cells. These bodies are usually movable starch ains. and mav convenientlv be termed *statoliths.* As long as that the Crustacean *Palæmon* is guided in keeping its dorso-ventral plane vertical, and dorsal surface upwards, by the stimulus of the statoliths resting on the internal surface of its otocysts.

In favour of their theory, Němec and Haberlandt have stated fully and well the general argument based on the distribution of statoliths. Thus, broadly speaking, statoliths are found to occur (in Phanerogams at least) in organs and parts of organs capable of gravitational stimulation. On the other hand, speaking generally, statoliths are absent where there is no power of curving geotropically. Thus in the older parts of stems, which have lost the power of curvature, the statoliths disappear from the endodermis.*

The presence of statoliths in the tip, and only in the tip of the root, is a striking fact, and one that is in general accordance with the theory set forth in the *Power of Movement in Plants*, and confirmed since the appearance of that work by more than one observer.† The same thing is true of the cotyledon of *Setaria* and *Sorghum*, the statoliths are practically confined to this part of the seedling in which the gravitational sensitiveness has been shown to reside.‡ For further details of the general argument from distribution we must refer to the writings of Němec and Haberlandt. Personally, I consider the general argument to be highly convincing, but this opinion seems not to be shared by others, and in any case it does not permit us to neglect experimental tests.

Experimental Evidence.

Němec lays some stress on the loss of geotropism following the destruction of the statoliths. He employs Pfeffer's method of embedding a seedling bean or pea in liquid plaster of Paris. In this way, when the gypsum sets, the seedling is prevented from growing, and for some unknown reason the treatment leads to the disappearance of the starch from the group of specialised cells at the tip of the root. According to Němec seedlings so treated fail to respond to gravitational stimulus. But the argument is not complete, it should be shown that the starchless roots are not also inhibited in their reaction to other stimuli. For instance, supposing the experiment was performed on seedlings of *Sinapis*, it should be ascertained whether the starchless radicles would or would not curve apoheliotropically, while remaining incapable of geotropic curvature.

I have tried a similar experiment on *Setaria* and *Sorghum*. It was discovered accidentally that the cotyledons lose a great part of their arch if kept for periods varying from 6—26 hours at a high temperare, *e.g.*, 33—38. Such seedlings show a marked loss of geotropic ovement.

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* Haberlandt, 'Pringsheim's Jahrbücher,' vol. 38 (1903), p. 451; and 'Ber. der Deutschen Bot. Ges.,' vol. 18 (1900), p. 264.

† Pfeffer, 'Annals of Botany,' vol. 8 (1894), p. 317; Czapek, 'Pringsheim's Jahrbücher,' 1895; F. Darwin, 'Linn. Soc. Journal,' vol. 35, 1902, p. 266.

‡ F. Darwin, 'Annals of Botany,' vol. 13, 1899, p. 567.

Exp. 201, March 3, 1902.

Eight specimens of *Sorghum vulgare* were kept for 21¾ hours at 7°, and others at 16·5°—19° C. They were then allowed to curve eotropically for 20h. 20m. when the "cool" lot showed an average rvature of 49·8°, while the "hot" specimens gave an average of 9°.

Exp. 204, March 4, 1902.

Twelve specimens of *Sorghum vulgare* in two equal lots for 21h. 5m. at 7° and at about 18°. Microscopic examination at the close of the xperiment showed considerable irregularity in the disappearance of arch, and the following table shows that the degree of geotropic rvature is more or less parallel to the amount of starch remaining.

1.	Starch in small quantities………	72° (geotropic curve).
2.	,, ,, ………	70°
3.	Less starch than 1 and 2 ………	33°
4.	Starch almost disappeared ……	20°
5.	,, ,, ……	28°
6.	,, ,, ……	11°

At first I was inclined to see in facts like these a striking con-the tip containing the statocytes have been removed, the capacity for geotropism returns with the regeneration of the statoliths. Even here—though the fact is a striking one—the argument seems to require the heliotropic test. For obviously the regeneration of the tip may mark the recovery of a generalised sensibility, and not merely the rehabilitation of the special gravitational mechanism. The same objection holds to some extent with regard to Haberlandt's* experiments on plants found to be starchless in winter. It should have been more definitely shown that they are heliotropically active though incapable of gravitational reaction.

The Tuning-fork Method.

In the autumn of 1901, I began a series of experiments by a method which was at the time a new one, but has been in the meanwhile published by Haberlandt.† It seems to me that Haberlandt's argument is open to the objection above set forth, and as it is an objection I have tried to meet, my results seem to be worth giving.

My point of view was that if gravitational sensitiveness is a form of contact-irritability (which must be the case if the pressure of the statoliths on the plasmic membrane is the critical event), then it might be possible to intensify the stimulus by vibration. I hoped, by applying vibration in a vertical plane to a horizontal seedling, to make the starch grains dance on the lateral walls, and by such repeated blows on the protoplasm to produce more active geotropic response.

The experiments were made with seedlings of *Sorghum*, *Setaria*, and *Panicum*. In the earlier trials entire seedlings were used, but they were found difficult to fix horizontally with sufficient accuracy, and I consequently employed cut hypocotyls cemented by means of melted cocoa-fat on cork supports, and kept damp in small metal boxes, each containing a strip of wet filter-paper.

The vibration was supplied by means of a tuning fork driven by an electric escapement. The fork was fixed in a horizontal plane so that the vibration was vertical. The amplitude of the vibration varied in different parts of the fork from 4 mm. to less than a millimetre. The rate was about 47 vibrations per second.

The general plan of the experiments was to attach a pair of metal boxes, one to each limb of the fork, each box containing four to six seedlings fixed approximately horizontal. Control boxes were placed on a support a few centimetres from the fork. It was found essential to insure an identical temperature for the experimental and control plants. The fork is set in motion and the experimental plants subjected to vibration for about 8—20 minutes. The control and experimental boxes are then placed on a klinostat to avoid further gravitational stimulus, and the angular curvature estimated after a few hours. The general result is clear, viz., that the plants subjected to vibration are more strongly curved. In other words, that vibration increases the gravitational stimulus.

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* Haberlandt, 'Pringsheim's Jahrbücher,' 1903, p. 472.

† *Loc. cit.* (1903), p. 489.

Precautions and Sources of Error.

In performing the experiment it is necessary to take scrupulous care that the control and experimental plants receive similar treatment. In the case of both it is necessary to cement the plants into their boxes without allowing any possibility of geotropism being induced before the boxes are fixed to the tuning fork. In all the later experiments the plants were cemented into their places vertically and received no gravitational stimulus until horizontal on the fork, or on the place prepared for the control plants, as the case might be. Any error arising for the want of this precaution in the earlier experiments was equally divided between the experimental and control plants.

The most serious source of doubt and error is the tendency in the grass seedlings to nutate in various directions; all that can be the tuning fork. In this case any acceleration in the line of the fork would be in favour of the "still" specimens.

Table I requires a few words of explanation. The third column gives the number of the experiment and simply refers to the original notes. The next column gives the time in minutes during which the plants were left horizontal. Then follow the temperature and the length of time during which the plants were left on the klinostat. When two readings occur under this column it means, of course, that the curvature was measured more than once: thus in experiment 74 the curvature was read after 4 hours' rotation, and again after 6h. 33m. from the time at which the plants were originally placed on the klinostat. The column "Shaken" gives the angular deviation from the horizontal of the plants which had been on the fork; their average curvature follows in a separate column. The last two columns give in the same way the actual observations and the average for the control plants. For the sake of brevity we use the terms "Shaken" and "Still" for the experimental and control plants. In columns "Shaken and "Still" the letter x occurs occasionally in brackets, thus (x): this means that nutation downwards or sideways had occurred. In the same way (5) indicates a downward or lateral nutation of 5°. In striking averages, I have counted the nutating specimens and also those which showed no geotropism, *i.e.*, remained horizontal. Thus, if the readings were 13, 15 (x) 17, the average would be $\frac{45}{4} = 11{\cdot}3$: or if it had been 13, 15, 0, 17, the average would have been the same. When the cases of nutation were very frequent, or where the curvature occurred very slowly, as in experiments 131, 140, I have omitted the average curvature from consideration, although I have allowed the readings to remain in the table.

The first thing that strikes one is that the *shaken* specimens show a greater curvature than the *still* ones in a large majority of cases.

Thus, taking the whole of the thirty cases in which the average curvatures are calculated, we have:—

Shaken, curvature greater in	25 cases.
,, equals or practically equals *still*	3
Still, greater than *shaken*	2

Out of these thirty cases, seven are second readings, *i.e.*, records of curvature in experiments in which readings had been taken a few hours previously. If these are omitted, we get:—

Shaken, curvature greater.........	19
,, equals *still*..................	2
Still, curvature greater	2

Finally, there are four cases in which the plants were left on the fork ropism.

haken.				Av.	Still.						Av.
6	18	18	10	22·4	35	25	10	25	18	10	20·5
0	23	45	40								
38	42	40	76	39·2	15	0	30	41	30	40	26
22	25	47	65								
30	12	25	12	16·5	20	0	7	12	0	0	6·5
33	26	30	8	23·4	28	5	12	8	0	18	11·8
..	..	..	..								
8	16	(8)	0	..	25	14	(80—90 lateral)				
30)	10	10	6	..	10	2	2	0	15	10	
45	28	..	..	26	13	(8)	6	7	..	..	8
55	12	..	..		5	8	15	10	..	..	
0	0	..	..	11	20	50	0	0	..	..	10·6
10	10	..	..	..	0	(10)	15	(5)	..	..	
0	15	7	11	9·3	6	6	10	(13	20)	16	6·3
40	..	..	..	33·3	17	20	17	..	..	..	18
10	..	..	..	22·8	0	20	18	..	..	..	18
15	..	..	..		18	30	22	..	..	..	
8	8	..	..	8·8	12	12	(15)	11	..	..	6·6
0	3	..	..		13	(4)	5	0	..	..	
0	0	..	..	..	8	8	0	0	..	..	
0	0	..	..	..	(7)	0	0	0	..	..	
26	25	..	..	19·4	15	17	10	23	..	..	12·5
12	22	..	..		3	4	10	18	..	..	

Table I—*continued.*

	Plant.	Ex.	Stim.	T.	Klino.	Shaken.						Av.	Still.						Av.
1902.			m.		h. m.														
Jan. 18...	*Set. ital.*...	133	20	21	7 21	80	75	82	69	..	..	71·1	65	52	80	28	..	..	51·4
						70	68	85	40	..	..		90	68	0	28	..	..	
,, 20..	*Set. ital.*...	134	15	19	5 44	40	95	85	80	..	..	76·2	80	45	80	80	..	..	75·1
						115	75	80	40	..	..		95	63	68	90	..	..	
,, 22...	*Set. ital.*...	136	10	20	4 55	30	30	20	10	..	..	28·2	32	12	12	14	..	..	30·9
						15	35	51	35	..	..		40	50	20	67	..	..	
,, 23...	*Set. ital.*...	140	5	..	5 13	5	(*x*)	(*x*)	0	..	..	..	11	9	(*x*)	1	..	..	
						11	(1)	..	..	..	..	..	(2)	5	0	0	..	..	
,, 24...	*Set. ital.*...	142	8	..	3 27	23	15	23	25	..	..	25·1	14	13	14	0	..	..	15·5
						36	53	19	7	..	..		19	22	26	16	..	..	
,, 27...	*Sorg. vulg.*..	144	8	19	3 11	5	8	0	(*x*)	..	..	5	5	5	10	1	..	..	7·3
						2	5	8	12	..	..		(*x*	*x*)	10	27	..	..	
					6 49	17	25	12	0	..	..	14·8	0	35	20	15	..	..	14
						13	10	31	10	..	..		(*x*)	0	5	37	..	..	
,, 28...	*Set. ital.*...	149	8	18	3 6	15	0	0	16	..	..	3·8	(*x*)	11	9	(*x*)	..	..	2·5
Feb. 5...	*Sorg. vulg.*..	167	9	..	3 15	11	8	13	5	..	..	11·4	2	9	9	6	..	..	6·6
						9	12	21	12	..	..		13	6	4	4	..	..	
					7 10	22	17	25	10	..	..	17·5	4	10	19	9	..	..	10·8
						10	22	20	14	..	..		18	12	10	4	..	..	
Feb. 6....	*Set. ital.*...	170	8	17	2 35	8	12	18	2	..	..	10·1	5	7	0	0	..	..	3·0
						10	14	10	7	..	..	..	9	3	0	(*x*)	..	..	
					7 20	18	21	27	15	..	..	17·4	8	10	3	0	..	..	5·5
						10	15	20	13	..	..	..	15	8	0	(*x*)	..	..	
,, 7....	*Sorg. nigr.*..	172	8	18	3 38	14	12	15	25	..	..	13·6	(28)	(10)	0	0	..	..	0·5
						8	18	10	7	..	..	..	(8)	4	(2)	(8)	..	..	
,, 13....	*Set.*	183	8	..	2 30	2	6	7	8	..	..	5·8	(5)	8	2	2	..	..	2·8
													0	0	0	10	..	..	
,, 14....	*Set.*	184	10	18	3 48	(3)	0	12	0	..	..	3·0	6	(15)	1	3	..	..	2·5

continued.

Shaken.				Av.	Still.						Av.
12	20	..	..	7·9	0	3	2	0	..	..	2·5
6	0	..	..	..	3	5	2	5	..	..	
13	6	..	..	7·9	3	5	6	0	..	..	3·7
4	(2)	..	..	..	3	7	2	0	..	..	
10	22	..	..	9·8	0	2	2	5	..	..	2·2
10	(2)	..	..								
19	26	..	..	11·6	5	5	13	12	..	..	8·8
11	(5)	..	..								
17	17	..	..	12·9	6	11	1	1	..	..	5·8
(1)	1	..	..	..	9	3	12	3	..	..	
23	16	..	..	15·6	7	16	0	7	..	..	7·5
(4)	2	..	..	..	10	7	11	2	..	..	

for long periods, *i.e.*, 40—82 minutes; these are, perhaps, not strictly comparable with the other experiments, though in three out of the four the *shaken* specimens were clearly more curved than the *still* ones. If these are omitted, we have:—

Shaken, curvature greater	15
,, equals *still*	2
Still, curvature greater............	2

The amount of curvature is not merely greater in a large majority of cases, but also differs by a considerable percentage.

Thus, taking the sum of the average curvatures in all thirty cases, we get:—

Still.	Shaken.	
403°·7	600°·8	or 100 : 148·8

Omitting the seven second readings (which give the sums—*still* 62·1, *shaken* 108·2), we get:—

Still.	Shaken.	
341°·6	492°·6	or 100 : 144·2

Finally, omitting the four cases of long exposure (which give—*still* 71·1, *shaken* 103·7), we get:—

Still.	Shaken.	
270°·5	388°·9	or 100 : 143·8

In another series of experiments the difference between the *shaken* and *still* plants was much smaller. But we have reason to believe that the failure depended on the small amplitude of vibration employed, for when, in the last four experiments of the series, a more ample vibration was adopted, there was once more a well-marked increase in geotropism in the *shaken* specimens.

In none of our experiments have we seen such striking results as those obtained by Haberlandt; it must, however, be remembered that his apparatus differs from, and is apparently more effective than, ours.

Control Experiments (Heliotropism).

The experiments were made in the same way as the last, except that the seedlings were vertical instead of horizontal, so that the starch would be made to vibrate on the basal instead of on the lateral walls of the cells. The boxes had glass lids to admit the light, which was given by incandescent gas. Care was taken that the distance from the light of the *still* specimens was the same as that of the *shaken* specimens.

Table II.—Heliotropism.

	Plant.	Ex.	Stim.	T.	Klino.	Shaken.	Av.	Still.	Av.
1901.					h. m.				
Dec. 14...	*Sorg. vulg.* ...	102	70	18—19	0 0	10 15 0 15	10·0	5 20 10 15	12·5
					3 25	8 15 12 (6)	8·8	14 4 0 26	11·0
„ 17...	*Sorg. vulg.* ...	109	90	21—23	0 0	12 5 25 10	13·0	0 12 8 0	5·0
					4 30	22 (5) 30 15	16·8	5 17 20 0	10·5
„ 18...	*Sorg. vulg.* ...	111	50	22—23	0 0	8 15 0 0	5·8	5 30 0 10	11·2
					4 5	38 32 30 28	32·0	70 45 30 48	48·2
1902.									
Jan. 10...	*Set. ital.*	126	58	23	5 10	67 50 22 ..	46·3	45 30 38 ..	37·6
„ 9...	*Sorg. vulg.*.....	127	65	22	4 5	25 30 12 ..	22·3	10 27 15 ..	17·3
Feb. 8...	*Sorg. vulg.*.....	173	60	22	2 28	0 13 3 7	5·8	8 12 5 8	8·2
					5 38	(*x*) 15 0 10	6·2	10 14 9 12	11·2
„ 21...	*Set. ital.*	188	61	..	3 23	4 15 7 12 22	12·0	8 10 10 11 2	8·2
Oct. 7...	*Set. ital.*	224	40	21	4 40	26 48 18 33	28·3	22 26 (8) 9	22·3
						20 15 30 36	..	38 25 28 30	
„ 8...	*Set. ital.*	225	37	22	5 18	22 15 25 17	17·0	5 38 10 0	14·4
						27 11 19 (5)	..	(3) 15 33 14	
„ 9...	*Set. ital.*	226	35	21	5 19	90 10 72 45	57·5	78 73 65 38	57·0
						48 98 42 55	..	17 30 85 70	
„ 10...	*Pan. mil.*	227	32	22	5 33	19 31 27 18	20·9	20 8 5 11	14·0
						23 0 32 17	..	28 15 11 14	
			Total, omitting 1st reading in Exps. 102, 109, 111.......				273·9		259

In three of the experiments (102, 109, 111) readings were taken before the plants were placed on the klinostat, and these readings are omitted in summing up the results :—

Curvature of *shaken* plants greater...	7 cases
„ *still* „ „ ...	4
Shaken, equal *still*........................	1

Thus there is a majority of wins in favour of the shaken plants, though it is not nearly so striking as in the geotropic experiments. When the *amount* of curvature is taken into consideration, the result is decisive :—

	Still.	Shaken.
Sum of averages	259·9	273·9
or as	100	105·4

The conclusion at which I arrive is that vibration does not materially increase heliotropic curvature, whereas it does increase geotropism. The following figures show in round numbers the difference between the geotropic and heliotropic experiments :—

Geotropic Curvature.

Still.	Shaken.
100	144

Heliotropic Curvature.

Still.	Shaken.
100	105

The inference I draw from this result is the same as that of Haberlandt, viz., that the increased geotropism of the *shaken* specimens is due to the increased stimulus produced by the vibration of the starch grains on the protoplasm of the lateral walls of the cells.

I desire to express my thanks to Miss Pertz, and to my assistant Mr. Elborn, for much valuable help.

n the Laws governing Electric Discharges in Gases at Low Pressures."

By W. R. CARR, B.A., University of Toronto. Communicated by Professor J. J. THOMSON, F.R.S. Received February 11,—Read March 5, 1903.

(Abstract.)

The experiments described in this paper were undertaken with the ject of determining the potential difference required to produce charge in a number of gases over a wide range of pressures, and ecially of ascertaining if the law enunciated by Paschen* was nerally applicable, provided the electric field in which the discharge k place was uniform.

The paper is divided into the following sections :—

(1.) Introduction.
(2.) Description of apparatus.
(3.) Experiments in air.
(4.) Experiments in hydrogen.
(5.) Experiments in carbon dioxide.
(6.) Spark potentials with different electrodes.
(7.) Minimum spark potentials.

discharges at low pressures in all probability did not take place along the shortest path between the plates, and it is inferred that the failure of his numbers to establish the applicability of Paschen's law at all pressures is due to his having always taken this shortest distance between the electrodes as a measure of the spark length.

In the present paper an account is given of an investigation on the potential difference necessary to produce discharges in a gas with a form of apparatus which ensured the passage of the discharge in a uniform electric field at all pressures. With this apparatus the spark potentials were determined in air, hydrogen, and carbon dioxide, for different distances between the electrodes, over a range extending considerably above and below the critical pressures. Electrodes of brass, iron, zinc, and aluminium, of the same size, were in turn used in the apparatus, but the readings obtained showed that the spark potentials were not influenced at any pressure by the size of the electrodes, provided the discharge took place in a uniform field.

The result of the investigation not only confirmed the truth of the law enunciated by Paschen for discharges in a gas at high pressures, but also demonstrated the applicability of the same law to the critical and to lower pressures. This law is summarised in the statement "that with a given applied potential difference, electric discharge in a uniform field in any gas is dependent solely on the constancy of the quantity of matter per unit cross-section between the electrodes."

It is shown that a general application of Paschen's law demands that the minimum spark potential must be a physical constant for each gas. A special set of observations gave the following values of this quantity for a number of simple and compound gases.

Gas.	Minimum spark potentials in volts.
H_2	278
O_2	455
H_2S	414
CO_2	419
N_2O	420
SO_2	457
C_2H_2	467

Adopting Strutt's value of 251 volts for nitrogen, the conclusion is drawn that the minimum spark potential is a property of the atom rather than the molecule of a gas, and it is shown that if H', N', O', etc., represent the spark potential constants in volts, corresponding to atoms of the gases H_2, N_2, O_2, etc., respectively, the minimum spark potential for any compound gas whose formula is $H_xN_yO_z$, etc., will be given by $xH' + yN' + zO'$ + etc. volts. It is pointed out that oxygen forms an exception to the general application of this law.

The latter part of the paper deals with the extension of Paschen's w to spark lengths much shorter than those actually used in the xperiments, and evidence is adduced in support of the conclusion at the law is applicable for discharges in a uniform field in any gas, s long as the spark length is greater than the diameter of the sphere f molecular action.

On the Optical Activity of Hæmoglobin and Globin." By ARTHUR GAMGEE, M.D., F.R.S., Emeritus Professor of Physiology in the Owens College, Victoria University, and A. CROFT HILL, M.A., M.B., late George Henry Lewes Student in Physiology. Received January 31,—Read February 12, 1903.

Introductory Observations.

All observations hitherto published concerning the optical activity f the albuminous substances have led to the conclusion that the odies thus designated, whether derived from the vegetable or the imal kingdoms, without a single exception, deviate the plane of characterised and the most striking members of this group are: Firstly, the hæmoglobins and their compounds. Secondly, the nucleoproteids and the nucleins.

In hæmoglobin, we have the example of a complex proteid, which differs from all other members of the albuminous group of bodies by its colour, by its marvellous power of forming easily dissociable compounds with oxygen and certain other gases, by the facility with which it admits of being crystallised and recrystallised and obtained free from all foreign mineral matters, by the startling manner in which its solutions fail to furnish any one of the reactions characteristic of albuminous substances in solution, *so long as the reagent has not effected a fundamental decomposition which has liberated the albuminous and coloured residues*. The researches of one of us have moreover lately shown that whilst hæmoglobin is a diamagnetic body, the iron-containing products of its decomposition by acids are not merely paramagnetic, but probably the most powerfully "ferromagnetic" organic bodies known to science.*

So complete a divergence thus exists in physical and chemical properties between hæmoglobin and the substances which are the immediate products of its decomposition, and which are doubtless linked together in it, that it appeared in the highest degree interesting to ascertain whether or not, in respect to optical activity, hæmoglobin would behave as an albuminous body proper and prove to be "lævogyrous." Having, if possible, determined this point, the subsequent step in the research would naturally be to determine the optical activity of the albuminous and coloured products of the decomposition of the hæmoglobin molecule.

1.—*Determination of the Optical Activity of Hæmoglobin.*

So far as the authors have been able to ascertain, the optical activity of solutions of coloured organic bodies has not yet formed the object of serious investigation. Landolt,† in the last edition of his authoritative work, which contains all the reliable results relating to the optical activity of organic bodies up to the date of its publication, mentions only one colouring matter as having been investigated, viz., the vegetable colouring matter hæmatoxylin, of which the alcoholic solution is said to be dextrogyrous. Nor is this neglect of the study of the optical activity of coloured solutions surprising when we consider the much greater difficulties which encounter the observer, in comparison to those attending the examination of colourless solutions.

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* A. Gamgee, "On the Behaviour of Oxy-hæmoglobin, Carbonic-oxide-hæmoglobin, Methæmoglobin, and certain of their Derivatives in the Magnetic Field, with a Preliminary Note on the Electrolysis of the Hæmoglobin Compounds," 'Roy. Soc. Proc.,' vol. 68, p. 503.

A. Gamgee, The Croonian Lecture for 1902, "On certain Chemical and Physical Properties of Hæmoglobin," 'Roy. Soc. Proc.,' vol. 70, p. 79.

† Dr. H. Landolt, 'Das Optische Drehungsvermögen Organischer Substanzen, &c., Zweite gänzlich umgearbeitete Auflage,' Vieweg u. Sohn, 1898.

The Method Employed in the Present Research.

When a powerful beam of white light is passed through a stratum 1 cm. thick of a solution of Oxy- or CO- hæmoglobin containing 0·9 per cent., the only region of the spectrum which is unabsorbed is that which extends from B to a little distance on the red side of D. It was therefore clear that the only light which could be employed in the work before us was monochromatic red light, and that in the place of one of the polarimeters commonly employed in our laboratories and whose adjustments only permit of their being employed with light of a definite wave-length (the half-shadow polarimeters of the Laurent type being adjusted for use with monochromatic sodium light), an instrument should be employed, the arrangements of which permit of observations with light of any desired wave-length.

In our first observations, we attempted to employ the lithium flame as our source of light, but we were unable to secure by this means either a sufficiently powerful or steady illumination. We subsequently employed, however, as a source of practically monochromatic red light, the light of an arc lamp which had traversed Landolt's filter for red rays

The polarimeter employed in these observations was a magnificent Lippich's "Halbschatten-Polarimeter," with tripartite field of vision, made by Schmidt and Haensch, of Berlin, and belonging to the Davy-Faraday Laboratory of the Royal Institution of Great Britain.

The Hæmoglobin Employed.[*]

The solutions of hæmoglobin employed for the determinations of which the results will be given below, were prepared with oxy-hæmoglobin of remarkable purity which had been obtained from the blood of the horse by following the best of the methods (the third method) described by Zinoffsky.[†]

Two preparations of hæmoglobin made on a large scale and at the interval of some months one of the other were employed. The preparation employed to make the solution of Oxy-hæmoglobin had been crystallised three times, the product of each successive crystallisation having been many times washed with ice-cold distilled water of which the purity was controlled by determining its electrical resistance. This solution contained 2·446 grammes of hæmoglobin in 100 c.c. For polarimetric observations this solution was diluted with an equal volume of distilled water, the dilute solution examined containing, therefore, 1·223 gramme of oxy-hæmoglobin in 100 c.c.

The preparation employed to make the solution of CO-hæmoglobin had been crystallised four times. The crystals of each successive crystallisation had been subjected to washing with pure distilled water as stated above, the solution of the washed crystals of the fourth crystallisation having been saturated with CO. This solution contained 1·84 grammes of dry CO-hæmoglobin. For polarimetric measurement this solution was diluted with an equal volume of distilled water; the dilute solution contained, therefore, 0·92 gramme of CO-hæmoglobin in 100 c.c.

Hæmoglobin, whether in Combination with Oxygen or Carbonic Oxide, is Dextrorotatory.

A. *Oxy-Hæmoglobin.*

The diluted solution of Oxy-hæmoglobin, previously referred to, was employed. This solution, containing 1·223 gramme of hæmoglobin in 100 c.c., was thoroughly saturated with oxygen before being subjected to polarimetric observation.

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[*] The part of Section 1 of this paper which follows has been recast, and the observations described under A and C added, since this paper was submitted to the Royal Society.—*March* 5.

[†] Zinoffsky, O., "Ueber die Grösse des Hæmoglobinmoleculs," 'Zeitschrift f. physiol. Chemie,' vol. 16 (1886), p. 23.

The tube employed in all the sets of observations measured 1 decimetre.

Three sets of observations were made.

	Observed angle.	Specific rotation $(\alpha)_C$.
1. Mean of first set of observations ...	+0°·12	+9°·8
2. ,, second set of observations	+0°·125	+10°·2
3. ,, third ,, ,,	+0°·1225	+10°·0

From the above observations we conclude that the specific rotation f Oxy-hæmoglobin for light of the mean wave length of C, $\alpha)_C = +10°·0 \pm 0°·2$.

B. *CO-Hæmoglobin.*

The diluted solution of CO-hæmoglobin, previously referred to, was mployed. This solution contained 0·92 gramme of CO-hæmoglobin n 100 c.c.

Two sets of observations were made with this solution; in the first et a tube 1 decimetre long, and in the second a tube 2 decimetres ong being employed.

	Length of tube.	Observed angle.	Specific rotation $(\alpha)_C$.
. Mean of first set of observations	1 decim.	+0°·098	+10°·65

oxygen; another portion was agitated with pure CO so as completely to expel the oxygen from its combination with hæmoglobin and replace it by carbonic oxide. In this way were obtained two solutions of hæmoglobin identical in so far as the quantity of colouring matter which they contained, but differing in the fact that in the one case the hæmoglobin was in combination with oxygen and in the other with CO. The solutions were examined in tubes of the same length under the same conditions of illumination. The result was to show that the rotations were identical in the two cases, having the mean value represented by the specific rotation $(\alpha)_C = +10^\circ{\cdot}0$.

It is to be remarked that the observations recorded under A and C were carried out subsequent to those on CO-hæmoglobin recorded under B. In the case, particularly, of observations A, the intensity and steadiness of the monochromatic red light employed was, in consequence of the experience previously acquired, more satisfactory than in observations B. We are therefore inclined to consider the numbers expressing the specific rotation of hæmoglobin which we have obtained as the result of observations A to be most worthy of confidence. We do not pretend that these numbers may not need slight modification as the result of future work, though we believe that they are a very close approximation to the truth.

2.—*Determination of the Optical Activity of Globin.*

Preyer gave the name of Globin to the albuminous product of the spontaneous decomposition of hæmoglobin, without, however, being able to furnish any precise account of its properties, its chemical composition, or its relationship to other albuminous bodies. A comparatively recent investigation which we owe to Fr. N. Schulz,* and the results of which have been substantially confirmed by Ivar Bang,† has placed us in possession of valuable and suggestive facts concerning the main albuminous product resulting from the decomposition of hæmoglobin. He has shown that when a solution of hæmoglobin is decomposed by the addition of small quantities of hydrochloric acid, it yields, as main products, 4·2 per cent. of hæmatin and 86·5 per cent. of a characteristic albuminous substance for which he retains the name of globin. He has shown that this substance belongs to the class of "the Histons," so that it would have been preferable, in our opinion, if Schulz had applied to his new body such a name as "Hæmato-Histon," which would have indicated both its origin and its affinities.

Schulz's method of preparing globin, as described by him, is essen-

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* Schulz, Dr. Fr. N., "Die Eiweisskörper des Hæmoglobins," 'Zeitschr. f. physiol. Chemie, vol. 24 (1898), p. 449.

† Bang, Ivar, "Studien über Histon," 'Zeitschr. f. physiol. Chem.,' 1899, p. 463.

ially as follows: to a solution of crystallised hæmoglobin, either pre-
ared by Hoppe-Seyler's method or by the ammonium sulphate method,
ilute hydrochloric acid is added in extremely small quantities, until
flocculent brown precipitate falls which is immediately dissolved by
he slightest excess of acid. The solution then no longer exhibits the
eautiful red colour of hæmoglobin, but has assumed a brown colour.
Not only, remarks Schulz, has its colour changed, but a complete
eparation has occurred between the albuminous and coloured con-
tituents of hæmoglobin. If to the solution, which has now a faint
cid reaction, about one-fifth of its volume of 80 per cent. alcohol be
dded and the mixture be shaken with ether, the whole [*sic*] of the
olouring matter is taken up by the ether, whilst the subjacent
queous-alcoholic, perfectly clear solution contains the decolourised
lbuminous matters. Schulz gives particular directions as to the
recautions which must be taken in order that the separation of the
thereal solution of the colouring matter should be complete, stating
hat a certain relation must exist between the proportions of water,
lcohol, and ether, which must be experimentally determined in each
ase. By the above process there is obtained a more or less brownish-
ellow solution, containing both alcohol and water and having a
aintly acid reaction. On neutralising this solution with ammonia, a
aintly yellow, coarsely flocculent precipitate falls. The latter is

20 c.c. of decinormal hydrochloric acid to effect the complete separation of globin from the colouring matter.

We found that agitation with ether, unless repeated several times, fails to remove all the colouring matter which is capable of removal in this way. Further, we found that even when, as a result of agitation with ether, the aqueous-alcoholic solution of globin is of the faintest straw colour, on being neutralised with ammonia the precipitated globin, which is at first colourless, assumes a somewhat reddish tinge, and when subsequently dissolved in water faintly acidulated with acetic acid the solution is much more deeply coloured than the original aqueous-alcoholic solution.

The following is the precise method which we followed in preparing the solutions employed in our polarimetric determinations:—

100 c.c. of a solution of four times crystallised hæmoglobin, containing 1·84 grammes of the substance, was diluted with 100 c.c. of distilled water and treated with 20 c.c. of decinormal hydrochloric acid. 44 c.c. of absolute alcohol were then added to the liquid, which was placed in a stoppered separating funnel and thoroughly agitated with its own volume of ether. The aqueous-alcoholic liquid having been separated from the supernatant ethereal solution of colouring matter was twice more agitated with fresh quantities of ether. By proceeding as we have described, the separation of the solution of globin occurred completely after the first agitation with ether, and the solution after the third agitation only possessed a faint straw colouration. In certain cases, the globin was separated according to the method of Schulz by precipitation with ammonia, the flocculent precipitate being subsequently dissolved in very weak acetic acid. In this manner was prepared the solution of globin which served for the first set of determinations recorded below. As it was impossible to obtain in this way solutions sufficiently colourless to allow of their rotation to be determined satisfactorily for light of the wave-length of D, this was done as in the case of hæmoglobin for light of the mean wave-length of C. In the second set of observations, the rotation of the aqueous-alcoholic solution resulting from the decomposition of hæmoglobin, after thorough agitation with ether, was determined.

Globin a Lævorotatory Substance.

Preliminary observations having shown that solutions of globin are optically active and lævogyrous, the following sets of observations were made with the object of determining the specific rotation of solutions of the substance.

1. A solution of globin in distilled water, but containing a little acetic acid, was examined with the arrangement for red light, as was used in the case of hæmoglobin. The solution contained 2·4 grammes

of globin in 100 c.c. It exhibited in the most characteristic manner the reactions of globin.

The tube employed measured 1 decimetre. The angle of rotation (mean of many determinations) was $-1°\cdot30$.

From the above data, it follows that in the case of this feebly acid solution of globin, containing 2·4 per cent., the specific rotation $[\alpha]_C = -54°\cdot2$.

2. The faintly straw-coloured solution obtained by the decomposition of hæmoglobin by means of dilute hydrochloric acid, the addition of alcohol and repeated agitation with ether, was placed in a shallow capsule in a current of air for some hours and afterwards on the water-bath at the temperature of 40° C. In this way all the ether and some of the alcohol were expelled. The perfectly clear straw-coloured solution, which had a density of 987·4 at 16° C., contained 0·98 gramme of solid matter in 100 c.c.

Monochromatic sodium light was employed in the polarimetric observations. The tube employed measured 1 decimetre. The angle of rotation (mean of many determinations) was $-0°\cdot64$.

From the above data, it follows that in the case of this feebly acid, aqueous-alcoholic solution of globin, containing 0·98 per cent. of solids the specific rotation, $[\alpha]_D = -65°\cdot5$. It may be pointed out that the greater part of the discrepancy between the results of the polarimetric measurements of the solution of separated globin and of the solution now under discussion is to be explained by the difference in the wave-length of the light, of which the rotation of the plane of polarisation was determined in the two cases.

General Conclusions.

The following are the conclusions to which we have been led by the experiments described in this paper:—

1. Hæmoglobin is a dextrogyrous albuminous body.

2. Globin, which is the principal, or as we are inclined to believe, the only albuminous product of the decomposition of hæmoglobin by highly dilute hydrochloric acid under the conditions determined by Schulz and confirmed by our own observations, behaves as a normal albuminous substance, in respect to its influence on the plane of polarisation of light, *i.e.*, it is a lævogyrous body.

Whilst the conclusions above stated are beyond question correct, we wish it to be understood that the numbers expressing the specific rotation of the bodies which we have examined must be looked upon as very close approximations and may need revision in the case of hæmoglobin by determinations carried out with a more perfectly monochromatic and intense light than that which we have employed, and in the case of globin by working with the substance in a purer condition than is possible in the actual state of our knowledge of this body.

We hope to be able to carry out these further investigations, and to direct our attention to the optical activity of the coloured products of the decomposition of the hæmoglobin molecule, especially hæmochromogen and hæmatin and their coloured derivatives.

In conclusion, we have to express our thanks to the Managers of the Davy-Faraday Laboratory of the Royal Institution for the facilities which they afforded us in carrying on the optical part of our work.

"On the Nucleoproteids of the Pancreas, Thymus, and Suprarenal Gland, with especial Reference to their Optical Activity."

By Arthur Gamgee, M.D., F.R.S., Emeritus Professor of Physiology in the Owens College, Victoria University, and Walter Jones, Ph.D., Associate Professor of Physiological Chemistry in the Johns Hopkins University. Received February 9,—Read February 12, 1903.

Part I.—Bibliographical and Critical.

In a research in which one of us was associated with Dr. A. Croft Hill, it was discovered that Hæmoglobin is a dextrorotatory body, whilst the interesting Histon-like albuminous substance Globin, which is obtained by the splitting up of Hæmoglobin under the influence of highly diluted hydrochloric acid, and of which the characters, no less than the mode of preparation, have only been known since the researches of Fr. N. Schulz, is a normally lævogyrous albuminous body.

These interesting observations naturally suggested the probability that the Nucleoproteids might, like Hæmoglobin, prove to be dextrogyrous, and the research of which the first results are contained in this paper is the outcome of this idea. The hypothesis has been fully confirmed, as will be shown in the sequel, and it has thus been proved that some of the members of a group of albuminous bodies of great importance in the life-history of the organism, are dextrorotatory bodies.

The preparation of nucleoproteids of such purity and especially so free from contaminating colouring matters as to yield solutions sufficiently transparent and colourless for polarimetric work, was a necessary preliminary to our special researches, and has led to the discovery of many facts of interest in relation to the chemistry of the nucleoproteids.

reliminary Remarks concerning the "Nucleoproteids" and "Nucleins" and the Sense in which the Latter Term is used in the Present Paper.

By the term Nucleoproteids, we designate complex, or rather ompound, albuminous substances which are the constituents of the cleated protoplasm of all the organs of the animal body, but pecially of the ductless, as well as of the secreting, glands. These dies are characterised by the large quantity of phosphorus which they ntain, by the constant presence of iron, and by the fact that under e influence of heat, by the action of acids, of alkalies, but especially pepsin and hydrochloric acid, acting at temperatures favourable to eir action, they split up into albuminous matters, and into so-called ue (to distinguish them from pseudo-) nucleins. The latter differ om the mother nucleoproteids which yielded them, by the fact at they result from the splitting-off of a fraction of the albuminous olecules which these contained in their pristine and native contion. These secondary, or we may say, degraded nucleoproteids, the nucleins," contain all the phosphorus originally present in the other substance.

By the action of caustic alkalies and heat, the nucleins yield as oducts of decomposition, albuminous matters, and the so-called nucleinic acids," bodies which vary in composition in the different appear to be in unison with the facts known to us (Schmiedeberg, Levene, W. Jones and G. H. Whipple, T. B. Osborne and I. F. Harris).[*]

Hammarsten,[†] to whose researches on the nucleoproteids and their relations to the nucleins we owe much of our knowledge of these bodies, would restrict the term "Nucleins" to the albuminous compounds of the nucleinic acids which remain undissolved after prolonged digestion with pepsin and hydrochloric acid. But this limitation appears to us undesirable and unphilosophical, and we think that the term nuclein, which it is convenient to retain, both for historical and descriptive reasons, should be applied to designate all those bodies resulting from the splitting-off of some, but only some, of the albuminous molecules originally forming part of the more complex nucleoproteid mother substance. It is in this sense that we shall in this paper employ the term nuclein, it being understood that every nuclein is to be considered a nucleoproteid, inasmuch as it is a compound of an albuminous body with a nucleinic acid or acids.

The Researches of Hammarsten on the Nucleoproteids of the Pancreas.

In his most interesting and suggestive paper published in the year 1894, Hammarsten gave an account of two nucleoproteids which he had obtained from the pancreas.

The first of these bodies he designated proteid-α. He ascertained that this body which, being soluble in water, is present in cold aqueous extracts of the pancreas, is precipitated by acetic acid, and that when boiled its solutions yield a coagulated albuminous substance, the substance remaining in solution being presumably nucleoproteid-β. Although Hammarsten fully recognised that the first or α-body was the mother substance, and that proteid-β was only a product of its decomposition, he devoted his attention to the latter, being actuated by the following reasons:—In the first place, his object being at that time to study the non-albuminous products of the pancreatic nucleoproteid, it appeared to him more simple and wiser to take as the starting-point of the investigation a material containing less albumin. The chief ground, however, for leaving the more interesting nucleoproteid provisionally uninvestigated was the great difficulty of obtain-ng it in any degree pure, attempts at purification being attended ith such loss that the yield was too small.

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[*] O. Schmiedeberg, 'Archiv f. experiment. Path. u. Pharmak.,' vol. 43 (1899), p. 57; P. A. Levene, 'Zeitschr. f. physiol. Chem., vol. 32 (1901), p. 541; W. Jones and G. H. Whipple, 'Amer. Jour. of Phys.,' vol. 7 (1902), p. 423. See particularly the recent paper by Thomas B. Osborne and Isaac F. Harris, "Die Nucleinsäure des Weizenembryos," 'Zeitsch. f. physiol. Chem.,' vol. 36, Heft 2 (September, 1902), p. 85.

[†] Olof Hammarsten, "Zur Kenntniss der Nucleoproteïde," 'Zeitschr. f. physiol. Chem., vol. 19 (1894), p. 19.

Hammarsten remarked that among the impurities most difficult to eparate was the blood-colouring matter, as well as another colouring natter which he believed to be produced by the action of the air on he nucleoproteid itself. Further, another impurity adhering to the ucleoproteid was found by Hammarsten to be trypsin, which he was nable to separate from it. He remarks, indeed, that the proteolytic ctivity of the substance is so intense that in no other way could he btain so powerfully acting a trypsin.

Having, for the reasons above stated, abandoned the study of the nteresting mother-substance, his nucleoproteid-α, Hammarsten then irected his attention to the β-body. This body, he did not seek to btain by the decomposition of the mother substance, of which it is a roduct, but by adopting the following method:—he boiled the finely omminuted and perfectly fresh pancreatic gland of the ox in water nd obtained, after filtration, a perfectly clear, faintly yellow solution, o which he added, after cooling, from 1 to 2 parts of hydrochloric cid, or from 5 to 10 parts of acetic acid per 1000 parts of the liquid. n this manner he obtained an abundant, white, flocculent precipitate. Ie dissolved the substance thus precipitated in water, with the aid of he least possible quantity of alkali and reprecipitated it by adding

Although unable to separate the reducing substance in a state of purity, he succeeded in preparing an Osazone of constant melting point and the characters of which agree with those of the osazone of a pentose, an observation which absolutely coincides with the researches of Kossel and Bang, which establish the presence of a carbohydrate nucleus in the nucleinic acids and the formation of pentoses when they are subjected to the hydrolytic action of dilute mineral acids and heat. Further, Hammarsten showed that when his nuclein was decomposed by heating with a 3 per cent. solution of sulphuric acid on the water bath, a crystalline sediment often separated which, after being purified, was analysed and shown to consist of guanine sulphate. Later, at Hammarsten's instigation, Ivar Bang, continuing the investigation, prepared from Hammarsten's nuclein, the nucleinic acid to which he ascribed the name of Guanylic Acid.

PART 2.—EXPERIMENTAL.

On the Nucleoproteid of the Pancreas and on Certain Characters of the Nucleins which are associated with, or derived from, it.

A. *The Nucleoproteid.*

Method of Preparation.

The finely divided pancreas of the pig was treated successively with 50 per cent. alcohol, 75 per cent. alcohol, and 95 per cent. alcohol, and finally subjected to the action of absolute alcohol and ether, with the object of dehydrating it. The material thus obtained was extracted with successive portions of a 5 per cent. solution of ammonium acetate, the united extracts were filtered, and the perfectly clear fluid was poured into four times its volume of weak alcohol. The precipitate thus formed was washed by decantation with a large amount of dilute alcohol, and finally dried with absolute alcohol and ether. The object of this series of procedures was to remove the colouring matter of the gland, which is somewhat soluble in dilute alcohol, more so in an alcoholic solution of ammonium acetate, but soluble to a very slight extent in an aqueous solution of ammonium acetate. These manipulations also remove a large amount of inorganic salts, and render the coagulable albuminous substances insoluble.

A 2 per cent. aqueous solution of this raw material had only a pale yellow colour, and it was found that it could easily be examined in a tube measuring 220 mm. with the polarimeter, monochromatic sodium light being employed. The polarimeter was a "Halbschatten-Polarimeter" made by Schmidt and Haensch of Berlin. *The result of the examination was to show that the solution contained a dextrorotatory substance. The solution, moreover, failed to give any indication of the presence of a reducing substance, even by prolonged boiling with Fehling's solution*, and as found to be rich in material which yields xanthine bases on ydrolysis with sulphuric acid.

The main portion of the gland substance, purified by the processes bove described, was treated with 20 parts of water, and to the filtered lution acetic acid was added, drop by drop. When a quantity of cid had been added sufficient to bring the amount of acid in the ntire solution to 1 per cent., a well-defined white, flocculent, precipi- ate separated. This precipitate of nucleoproteid was separated by he centrifuge, suspended in water, and treated with an extremely ilute solution of ammonia, drop by drop, and the reaction of the quid continuously tested with litmus. A very small amount of alkali as needed to neutralise the adherent acetic acid, when the solution ecame neutral and remained so until approximately twice as much mmonia had been used as had been required to completely dissolve he nucleoproteid. Evidently, the nucleoproteid is, at least, a dibasic cid, whose acid ammonium salt is soluble in water and neutral to tmus.

Purification of the nucleoproteid was effected by alternate solution ammonia and precipitation with a minimal quantity of acetic acid. he final solution was poured into five volumes of 95 per cent. alcohol, ashed repeatedly by decantation with excessively large quantities of 5 per cent. alcohol and ether, and then placed in an exsiccator over

B. *Nuclein accompanying, and probably resulting from, the Nucleoproteid.*

Method of Preparation.

The aqueous extract of the purified gland substance to which acetic acid had been added until it contained 1 per cent. of the latter, and from which the nucleoproteid had thus been separated, was treated with 20 per cent. acetic acid added a drop at a time. When the liquid contained 2 per cent. of the acid not the slightest precipitation had occurred. Continued addition of acetic acid, however, soon caused a turbidity, and when the acidity reached 5—6 per cent., a well-defined flocculent precipitate fell. This precipitate, which we shall call nuclein, was separated by means of the centrifuge and, at a great cost of material, was twice washed with water, the washings being separated by centrifugalising. The washed nuclein was suspended in water, and solution of ammonia added cautiously, one drop at a time; when the nuclein was completely dissolved, the reaction of the liquid was still acid to litmus. This solution was poured into four volumes of 95 per cent. alcohol, and the precipitated nuclein washed and dried by the methods described in the case of the nucleoproteid.

The fluid from which the "nuclein" had been precipitated, as has been stated, was now poured into four volumes of alcohol, and the precipitate thus thrown down was washed and dehydrated by the action of alcohol and ether. This preparation, which is necessarily very impure and especially rich in organic salts, will be described and referred to as "residual material."

Thus, by fractional precipitation with acetic acid, in the presence of inorganic salts, we have obtained three preparations. The nucleoproteid, which is doubtless the body which Hammarsten denominated Proteid-α, is almost insoluble in pure water, but may be dissolved by minute quantities of ammonia and caustic soda. The body which we have termed nuclein, to indicate our opinion of its relation to the first substance, is soluble in water with the greatest ease.

By the addition of a trace of copper sulphate to a solution of the nucleoproteid in caustic soda a fine pink colour is produced, but not a shade of violet makes its appearance until a comparatively large amount of copper solution has been added, a reaction which resembles closely "the biuret reaction" with the proteoses. The "nuclein" by similar treatment gives only the faintest pink colour, the violet shade being observed even when a very small amount of copper sulphate is used, while the "residual material" produces a violet colour from the beginning.

It has recently been shown by one of us* that the nucleoproteid of ie pancreas, prepared in substantially the same manner as the pre-arations employed in the present research, yields, when subjected to ydrolytic treatment, two of the xanthine bases, viz., guanine and lenine, and in a ratio which closely approximates four equivalents f the former to one of the latter. The "nuclein" and "residual aterial" of the present research were also found to yield xanthine ases on hydrolysis with sulphuric acid. All the three preparations nder discussion contain phosphorus, all are completely precipitated om aqueous or faintly alkaline solutions by the addition of a trace f hydrochloric acid, and all yield precipitates when their neutral olutions are boiled.

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* Walter Jones and G. H. Whipple, "The Nucleoproteid of the Suprarenal Gland," 'Amer. Jour. of Phys.,' vol. 7 (1902), p. 423.

Optical Properties of the Nuclein.

We had convinced ourselves by the following experiment that the pecific rotation of the substance which we have denominated nuclein" would be found to be greater than that of the nucleo-roteid, before we had the opportunity of making a careful optical xamination of the former substance.

A perfectly neutral solution of the neucleoproteid was prepared by reating some of the substance with water and an insufficient amount f ammonia to effect complete solution. The filtered fluid, examined ith the polarimeter in a 200 mm tube gave a rotation of 1° 46′

tion, we have to remark that we noticed several times that very slight lævorotatory filtrates were obtained when hydrochloric acid was used for precipitating the proteid, and especially when the acid fluid was allowed to remain in contact with the precipitate. Presumably, the negative rotation is due to an optically negative acid albumin being formed, which is soluble in the dilute hydrochloric acid.

As in the case of the nucleoproteid, a solution of the nuclein yields a coagulum on heating, and the rotation of the solution is not appreciably changed. This would lead one to assume the existence of a nuclein of which the specific rotation is greater than + 64°·4. It can easily be proved that such a substance exists in the preparation which we have designated "residual material."

A weighed amount of this substance was dissolved in a measured volume of water. The solution was examined with the polarimeter, treated with hydrochloric acid, and the amount of matter determined in the filtrate, which was found to be optically inactive. The following data were obtained:—

Weight of substance taken	0·520	gramme.
Weight of optically inactive matter ...	0·269	„
Weight of optically active matter	0·251	„
Volume of solution	25 c.c.	
Observed angle	+ 1° 38′	
Length of tube	200 mm.	

$$[\alpha]_D = +81^\circ{\cdot}1.$$

C. Hammarsten's Preparation.

As we have already explained, Hammarsten's so-called nucleoproteid-β, which is obtained from an extract of pancreas made by boiling the finely comminuted gland in water must, *ipso facto*, be a nuclein. The results which we had obtained and which have been described, made it highly desirable that we should make an optical examination of this substance also. By slight departures* from the method described by Hammarsten, which were absolutely necessary to remove the colouring matter, but which cannot possibly have exercised any influence on the chemical nature of the product, we were able to prepare a nuclein which must have been identical with Hammarsten's preparation (nucleoproteid-β). The substance which we obtained is soluble in water, and gives a violet biuret reaction. Its solution was comparatively highly coloured, but possessed so great a rotatory power that fairly satisfactory polarimetric observations could be made in solutions of great dilution. The substance is dextrorotatory. The following data were obtained :—

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* We used ammonia for redissolving the nuclein, instead of a fixed alkali employed by Hammarsten. We also finally poured an aqueous solution of the nuclein into 95 per cent. alcohol, and washed by decantation with absolute alcohol and ether.

Weight of substance	0·200 gramme.
Volume of solution	25 c.c.
Observed angle (mean of eight readings) ...	0° 47′
Length of tube....................................	100 mm.

$$[\alpha]_D = +97^\circ\cdot 9.$$

On the Nucleohiston of the Thymus Gland.

It would seem quite easy to obtain this substance in any desired quantity by following the very simple method which Lilienfeld described in twenty lines.*

This method leads, however, to a product whose solutions are highly opalescent, and an optical examination could not be thought of. The cloudiness is so persistent, that for a long time we were inclined to believe it to be a property inherent in the substance. We finally succeeded, however, in obtaining solutions almost as colourless and transparent as distilled water. It is only necessary to extract Lilienfeld's preparation with a 5 per cent. solution of ammonium acetate and filter. The fluid filters very slowly, but perfectly clear and con-sheep are identical, and scarcely differ in chemical composition from the nucleoproteid of the pancreas prepared substantially by the same method as that which has served for the researches described in this paper.

The following table exhibits the results of the ultimate analyses of these bodies, and, for purposes of comparison, the analyses made by Hammarsten of his preparation is also given:—

	Nucleoproteid of suprarenal gland of sheep.	Nucleoproteid of suprarenal gland of ox.	Nucleoproteid of pancreas of pig.	Hammarsten's preparation.
C	46·22	46·81	45·83	43·62
H	6·10	6·38	6·26	5·45
P	4·70	4·72	5·05	4·48
N	17·92	17·85	17·42	17·39

As closely as the analytical processes at command could determine, the nucleoproteids of the pancreas and the suprarenal gland yield guanine and adenine in the same relative proportions, and these appear to indicate that one molecule of a nucleinic acid, or of a nucleoproteid, may yield two different xanthine bases.

We must refer the reader to the paper quoted above for a description of a method of separating the nucleoproteid of the suprarenal gland. As is well known, the characteristic physiologically active constituent of this gland forms a dark brown pigment when exposed in aqueous solution to the oxidising action of the air. Aqueous extracts of the gland are therefore always highly coloured, and this colouring matter places great difficulties in the way of the preparation of substances from the gland which are intended for optical examination. While, therefore, the work on the nucleoproteid of the suprarenal gland is not as satisfactory as we could desire, it can nevertheless be stated most positively that this nucleoproteid also is dextrorotatory.

The method of isolation which we employed does not differ essentially from that employed in the research already referred to, except that the gland was extracted several times with acetic acid before removing the nucleoproteid. A substance was finally obtained which is too highly coloured for accurate polarimetric determinations, but which, even in the necessarily high dilutions which could alone be used, could easily be shown to be dextrorotatory.

The following data were obtained:—

Weight of substance	0·199 gramme.
Volume of solution	25 c.c.
Observed angle	+ 0° 23′
Length of tube	100 mm

$$[\alpha]_D = +48^\circ\cdot1.$$

The value of this rotation is liable to revision, but its direction is beyond question.

Before formulating the general conclusions which, it appears to us, may legitimately be deduced from the researches of which an account has been given in this paper, we may sum up our work in the following manner:—

Summary.

We have, in this paper, described six substances obtained from various glands and have given methods by which several of these may be isolated and obtained sufficiently free from colouring matters to admit of exact polarimetric determinations.

All six of these substances yield on hydrolysis, albuminous bodies, phosphoric acid, and purin derivatives, and all contain iron in stable combination; they are, therefore, all nucleoproteids in the wide sense of the term.

The methods of preparation were such as to exclude all dextrorotatory substances which are not of a proteid nature, and all preparations were shown to be free from substances which reduce Fehling's solution even on prolonged boiling. Nevertheless, all these substances were found to be dextrorotatory, having specific rotations for light of the wave-length of D which vary from 37°·58, that of the nucleohisenquiries. We trust, therefore, that during the next few months we may be permitted to work out so far as we are able, the problems which have been suggested by the new facts recorded in this paper.

Supplementary Bibliographical Note.

Since the above paper has been in print, it has come under the notice of one of us that the late Professor Alexander Schmidt, of Dorpat, in his published researches on the coagulation of the blood,* drew attention to the fact that among the soluble constituents of protoplasm was a body to which he gave the name of "Cytoglobin," and which he found to be dextrogyrous. So far as we are aware, this observation of A. Schmidt has never been noticed or quoted, either by systematic writers on physiological chemistry or by those who have devoted their attention to, or written upon, the subject which formed the life-work of the Dorpat professor. There can be no question that Schmidt's cytoglobin was an exceedingly impure mixture of nucleoproteids, an opinion which is based upon the fact that his substance contained 12·52 per cent. of ash, and that, on ultimate organic analysis, the amount of carbon found was 56·36 per cent., as compared with 45·83, the percentage of carbon in the nucleoproteid of the pancreas. Still the fact remains that this indefatigable worker, whose suggestive writings have been too little read, left data which prove that the so-called "Cytoglobin" was nucleoproteid in nature, though in no sense a definite proximate principle, and that this impure mixture of nucleoproteids was characteristically dextrogyrous.

March 4, 1903. A. G.

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* Alexander Schmidt, 'Zur Blutlehre,' Leipzig, Verlag v. F. C. W. Vogel, 1892. Refer to the chapter entitled "Ueber den in Wasser löslichen Bestandtheil des Protoplasmas," &c. (pp. 127—142); 'Weitere Beitraege zur Blutlehre' (nach des Verfassers Tode herausgegeben). Wiesbaden, J. F. Bergmann, 1895. Refer to the chapter entitled "Zur Kenntniss des Protoplasmas und seiner Derivate" (pp. 201—249).

"A Note on a Form of Magnetic Detector for Hertzian Waves, adapted for Quantitative Work." By Dr. J. A. Fleming, F.R.S., Professor of Electrical Engineering in University College, London. Received February 11,—Read March 5, 1903.

The known power of electrical oscillations to demagnetise iron or steel was first applied in the construction of a detector of Hertzian waves, as far as the author is aware, by Mr. E. Rutherford.* The power possessed by electrical oscillations to annul the magnetic hysteresis of iron was discovered by Mr. G. Marconi and applied by him in the construction of his ingenious and extraordinarily sensitive Hertzian wave detector, for use in connection with wireless telegraphy.†

The following note describes a form of magnetic Hertzian wave detector, which has been constructed by the writer for the purpose of quantitative experiments in connection with Hertzian waves.

Every one who has experimented with a Hertzian oscillator, or electric wave radiator in any form, involving a spark gap, is well aware of the immense difference in the radiative power produced by slight alterations in the nature of the spark or the spark balls, and has surrounding it. The amount of demagnetisation was detected by means of a magnetometer. In this form, it has been much used in experimental work, but it was not a telegraphic receiver.*

In the sensitive telegraph receiver invented by Mr. Marconi the change in magnetisation of the iron, due to the temporary abolition of hysteresis, is detected by the production of a sound in a telephone connected to a secondary coil surrounding the iron.

After trying various forms, the writer has found that a convenient magnetic detector for Hertzian waves can be constructed in the following manner :—

On a pasteboard tube, about ¾ of an inch in diameter and 5 or 6 inches long, are placed six bobbins of hard fibre, each of which contains about 6000 turns of No. 40 silk covered copper wire. These bobbins are joined in series, and form a well-insulated secondary coil, having a resistance of about 6000 ohms. In the interior of this tube are placed seven or eight small bundles of iron wire, each about 6 inches in length, each bundle being composed of eight wires, No. 26 S.W.G. in size, previously well paraffined or painted with shellac varnish. Each little bundle of iron is wound over uniformly with a magnetising coil formed of No. 36 silk-covered copper wire in one layer, and over this, but separated from it by one or two layers of gutta-percha tissue, is wound a single layer of No. 26 wire, forming a demagnetising coil. This last coil is in turn covered over with one or two layers of gutta-percha tissue.

The magnetising or inner coils are connected in series with one another, so that when a current passes through the whole of them, it magnetises the whole of the wires in such a manner that contiguous ends have the same polarity. The outer or demagnetising coils are joined in parallel. Associated with this induction coil is a rotating commutator, consisting of a number of hard fibre discs secured on a steel shaft, which is rotated by an electric motor about 500 times a minute. There are four of these fibre discs, and each disc has let in its periphery a strip of brass, occupying a certain angle of the circumference. These wheels may be distinguished as Nos. 1, 2, 3, and 4. The brass sector of No. 1 occupies 95° of its circumference; the brass sectors of Nos. 2 and 3 occupy 135° of their circumference; and that of No. 4 disc 140° of its circumference. Four little springy brass brushes make contact with the circumference of these wheels, and therefore serve to interrupt or make electric circuits as the disc revolves. The function of the disc No. 1 is to make and break the circuit of the magnetising coils placed round the iron bundles, and thus by applying a magnetising current to magnetise them during a portion of one period of rotation of the disc, and leave them magnetised in virtue of magnetic retentivity during the remaining portion. The function of discs 2 and 3 is to short-circuit the terminals of the secondary coil of the bobbin during the time that the magnetising current is being applied by disc No. 1. A sensitive movable coil galvanometer is employed in connection with the secondary coil, one terminal of the galvanometer being permanently connected to one terminal of the secondary coil, and the other terminal connected through the intermittent contact made by the disc No. 4. This disc No. 4 is so set that during the time that the secondary coil is short-circuited, and whilst the battery current is being applied to magnetise the iron wire bundles, the galvanometer circuit is interrupted by the contact on disc No. 4.

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* Note added March 7th. A general term seems to be required to include all forms of wave-detecting devices. The author suggests the word *kumascope* (from κῦμα, a wave) for this purpose. Uncouth phrases, such as *anticoherer* or *self-decohering-coherer*, which have crept into use in connection with Hertzian wave telegraphy, would be rendered unnecessary.

The operations which go on during one complete revolution of the discs are as follows:—First the magnetising current of a battery of secondary cells is applied to magnetise the iron bundles, and during the time this magnetising current is being applied, the terminals of the fine wire secondary coil are short-circuited and the galvanometer is disconnected. Shortly after the magnetising current is interrupted the secondary bobbin is unshort-circuited, and an instant afterwards instance, to show in a very marked manner the great effect of slight differences in the surface of the spark balls. If a steady series of sparks from an induction coil is passed between the spark balls of a Hertz linear radiator, it will produce a steady deflection on a galvanometer connected with the above-described receiver placed at a distance. If the balls are then polished, the galvanometer deflection immediately increases considerably. If, on the other hand, the balls are slightly smeared with oil, the galvanometer deflection decreases. If the radiator is approached to the receiver, or withdrawn from it, corresponding variations in the galvanometer deflection take place.

Such an instrument will probably be found of great use in connection with the design of radiators and transmitters for Hertzian wave wireless telegraphy. Up to the present it has been generally difficult to ascertain whether an improvement in the signalling is due to an accidental increase in sensitiveness in the coherer, or to any alteration or change made in the transmitter.

Similarly, the instrument promises to be of considerable use in the investigation of the transparency or opacity of various substances to Hertzian waves, not merely qualitatively, but in the determination of a coefficient of absorption. Preliminary experiments of this description made with the above-described instrument seem to promise for it a field of practical utility, both in the laboratory and in connection with Hertzian wave telegraphy.

In the numerous experiments which finally resulted in the construction of the above-described form of wave detector, it was found to be essential to have the iron core in the form of a number of small bundles of iron wire, each wound over with its own magnetising and demagnetising coil. No good results could be obtained when the iron core was in the form of a large bundle, say half an inch in diameter, and enveloped by a single magnetising and demagnetising coil.

Another condition of success is the short-circuiting of the fine wire secondary coil during the time of magnetisation of the core. The core can be indefinitely increased in size, provided the augmentation of mass is obtained by multiplying small individual cores, each consisting of not more than eight or ten fine iron wires, and each wound over with a separate magnetising and demagnetising coil. The electromotive force in the secondary coil can in this manner be increased as much as is desired, and a very sensitive wave detector produced. The commutator can be driven either by an electric motor or by any other source of power.

In conclusion, I have pleasure in mentioning the intelligent assistance rendered to me by Mr. A. Blok in the experiments conducted in connection with this appliance.

"A New Form of Self-restoring Coherer." By Sir OLIVER LODGE, F.R.S. Communicated verbally March 5,—Received in Manuscript March 18, 1903.

On the general subject of the detection of Hertzian waves the writer took the opportunity of the discussion on Dr. Fleming's paper (p. 398), to describe briefly the latest form of coherer, which Dr. Muirhead and he had brought out and always now employed in connection with space telegraphy, and which their assistant Mr. E. E. Robinson had helped to work out. It might be described as a development of the mercury form of coherer described some years ago by Lord Rayleigh, and again in a modified fashion by Mr. Rollo Appleyard. In Lord Rayleigh's form this consisted of a pool of mercury cut across with a paraffined knife, and the two half pools connected to a battery and key. As soon as the key was depressed so as to throw a few volts on to the intervening film of oil, the electrostatic pressure seemed to squeeze the oil out, and the pools of mercury became one.

Needle points dipping in oil and mercury were tried as practical coherers, the points being pulled out electromagnetically every time a signal arrived. Rotating forms of contact for automatic decoherence were also tried in various forms, and ultimately the method took the picked up and recorded precisely as they are emitted, as has been tested by intercalating a siphon recorder in a much diluted tapping circuit at the sending end, so as to get a record with which to make comparison. The traces obtained at the two ends are identical to a surprising degree.

The mercury level has an adjustment which is easily made. One precaution is to keep the rim of the wheel clear of dust, which is done by a cork or leather pad pressed lightly against it by a spring.

The instrument is not at all sensitive to tremor, and requires no particular delicacy of adjustment. The wheel has to be positive, the mercury negative.

A telephone in circuit, through a transformer or otherwise, affords an easy method of discriminating the signals by ear. The speed of the wheel gives another convenient adjustment to suit various circumstances.

"On Central American Earthquakes, particularly the Earthquake of 1838."

By Admiral Sir John Dalrymple Hay, Bart., G.C.B., F.R.S. Received March 6,—Read March 19, 1903.

The very interesting report of Dr. Tempest Anderson and Dr. J. S. Flett on "Recent Volcanic Eruptions in the West Indies," induces me to suggest that inquiries should be made in Colombia and in Central America as to disturbances in those regions, in order to trace the connection existing between the earthquakes and volcanic eruptions which are there so prevalent, and those in the West Indies.

Doubtless Mr. Welby, who has just returned to his post at Sta. Fé de Bogotá, might be able to obtain thence some information on this matter.

In the British Association Report, of 1858, upon Earthquakes, those of 1835 and of 1841 are given; but, unfortunately, no information is given as to the effect of those disturbances from any British source.

The British Association in both cases seem to have been mainly indebted for their information to the Académie, and hence the effects in Martinique and Guadeloupe are quoted, but nothing is reported from our West India Islands.

One of the most terrible disturbances in its effects, that of 1838, is not even alluded to. The only record of it is in the work of E. G. Squier, late Chargé d'Affaires from the U.S. of America to the Republics of Central America (2 vols., Appleton, New York, 1852), who was commissioned by his Government to report upon that region in reference to the Nicaraguan Canal. Vol. 1 has the map; vol. 2, pp. 114, 115, and p. 162 alludes to the eruptions of Cosequina in 1838, nd in describing the earthquakes and disturbances and eruptions of 835, speaks of their effects in New Granada, Nicaragua, Popayan, Bogotá, Sta. Marta, Caracas, Hayti, Curaçao, and Jamaica.

I think it may be of interest to record my personal observations of he earthquakes and eruptions of 1838 as given in the log of H.M.S. "Imogene," Captain (afterwards Sir Henry) Bruce, in which I was hen serving.

On Friday, November 16, 1838, noon, Lat. 6° 2′ N., Long., 82° 9′ W. Rio Lejo (to which we were bound from Panama) bearing N. 38, W. 490 miles. At 10 P.M. we felt the shock of an earthquake.

Tuesday, November 20, 1838, at 3.5 A.M., noon of that day, Lat., 9° 17′ N., Long. 85° 2′ W. Three heavy shocks in succession. Calm c. r. The topsails were lowered on the cap, and the ship lay ill daylight, uncertain whether she had struck on a wreck or whether he was damaged. She rolled over to the limit of her capacity on three successive occasions.

At daylight (6 A.M.) all hands were sent for to see an extraordinary spectacle. The sea was white, as if it had been mixed with marl, and, as far as the eye could reach, the sea was covered with fish of various kinds stunned by the concussion. The boats picked up a sufficient supply for the ship's company, principally albacore, bonita, and dolphin. Some turtle were also captured.

We anchored in Rio Lejo on November 25, and remained there

"The Emanations of Radium." By Sir WILLIAM CROOKES, F.R.S.

Received March 17,—Read March 19, 1903.

A solution of almost pure radium nitrate which had been used for spectrographic work, was evaporated to dryness in a dish, and the crystalline residue examined in a dark room. It was feebly luminous.

A screen of platinocyanide of barium brought near the residue glowed with a green light, the intensity varying with the distance separating them. The phosphorescence disappeared as soon as the screen was removed from the influence of the radium.

A screen of Sidot's hexagonal blende (zinc sulphide), said to be useful for detecting polonium radiations, was almost as luminous as the platinocyanide screen in presence of radium, but there was more residual phosphorescence, lasting from a few minutes to half an hour or more according to the strength and duration of the initial excitement.

The persistence of radio-activity on glass vessels which have contained radium is remarkable. Filters, beakers, and dishes used in the laboratory for operations with radium, after having been washed in the usual way, remain radio-active; a piece of blende screen held inside the beaker or other vessel immediately glowing with the presence of radium.

The blende screen is sensitive to mechanical shocks. A tap with the tip of a penknife will produce a sudden spark of light, and a scratch with the blade will show itself as an evanescent luminous line.

A diamond crystal brought near the radium nitrate glowed with a pale bluish-green light, as it would in a "Radiant Matter" tube under the influence of cathodic bombardment. On removing the diamond from the radium it ceased to glow, but, when laid on the sensitive screen, it produced phosphorescence beneath, which lasted some minutes.

During these manipulations the diamond accidentally touched the radium nitrate in the dish, and thus a few imperceptible grains of the radium salt got on to the zinc sulphide screen. The surface was immediately dotted about with brilliant specks of green light, some being a millimetre or more across, although the inducing particles were too small to be detected on the white screen when examined by daylight.

In a dark room, under a microscope with a $\frac{2}{3}$-inch objective, each luminous spot is seen to have a dull centre surrounded by a luminous halo extending for some distance around. The dark centre itself appears to shoot out light at intervals in different directions. Outside the halo, the dark surface of the screen scintillates with sparks of light. No two flashes succeed one another on the same spot, but are scattered over the surface, coming and going instantaneously, no movement of translation being seen.

The scintillations are somewhat better seen with a pocket lens magnifying about 20 diameters. They are less visible on the barium platinocyanide than on the zinc sulphide screen.

A powerful electro-magnet has no apparent effect on the scintillations, which appear quite unaffected when the current is made or broken, the screen being close to the poles and arranged axially or equatorially.

A solid piece of radium nitrate is slowly brought near the screen. The general phosphorescence of the screen as visible to the naked eye varies according to the distance of the radium from it. On now examining the surface with the pocket lens, the radium being far off and the screen faintly luminous, the scintillating spots are sparsely scattered over the surface. On bringing the radium nearer the screen the scintillations become more numerous and brighter, until when close together the flashes follow each other so quickly that the surface looks like a turbulent luminous sea. When the scintillating points are few there is no residual phosphorescence to be seen, and the sparks succeeding each other appear like stars on a black sky. When, however, the bombardment exceeds a certain intensity, the residual phosphorescent glow spreads over the screen, without, however, inter-

and produce phosphorescence when brought near the screen. On turning the lens to the, apparently, uniformly lighted edge of the screen close to the finger, the scintillations are seen to be closer and more numerous; what to the naked eye appears like a uniform "milky way," under the lens is a multitude of stellar points, flashing over the whole surface. A clean finger does not show any effect, but a touch with a soiled finger is sufficient to confer on it the property. Washing the fingers stops their action.

It was of interest to see if rarefying the air would have any effect on the scintillations. A blende screen was fixed near a flat glass window in a vacuum tube, and a piece of radium salt was attached to an iron rocker, so that the movement of an outside magnet would either bring the radium opposite the screen or draw it away altogether. A microscope gave a good image of the surface of the screen, and in a dark room the scintillations were well seen. No particular difference was observed in a high vacuum; indeed, if anything, the sparks appeared a trifle brighter and sharper in air than in vacuo. A duplicate apparatus in air was put close to the one in the vacuum tube, so that the eye could pass rapidly from one to the other, and it was so adjusted that the scintillations were about equal when each was in air. The vacuum apparatus was now exhausted to a very high point, and the appearance on each screen was noticed. Here again I thought the sparks in the vacuum were not quite so bright as in air, and on breaking the capillary tube of the pump, and observing as the air entered, the same impression was left on my mind; but the differences, if any, are very minute, and are scarcely greater than might arise from errors of observation.

It is difficult to form an estimate of the number of flashes of light per second. But with the radium at about 5 cm. off the screen they are barely detectable, not being more than one or two per second. As the distance of the radium diminishes the flashes become more frequent, until at 1 or 2 cm. they are too numerous to count.

[*Added March* 18.—On bringing alternately a Sidot's blende screen and one of barium platinocyanide, face downwards, near a dish of "polonium" sub-nitrate, each became luminous, the blende screen being very little brighter of the two. On testing the two screens over a crucible containing dry radium nitrate, both glowed; in this case the blende screen being much the brighter. Examined with a lens, the light of the blende screen was seen to consist of a mass of scintillations, while that of the platinocyanide screen was a uniform glow, on which the scintillations were much less apparent.

The screens were now turned face upwards so that emanations from the active bodies would have to pass through the thickness of card before reaching the sensitive surface. Placed over the "polonium" either screen showed any light. Over the radium the platinoyanide screen showed a very luminous disc, corresponding with the pening of the crucible, but the blende disc remained quite dark.

It therefore appears that practically the whole of the luminosity n the blende screen, whether due to radium or "polonium," is occaioned by emanations which will not penetrate card. These are the manations which cause the scintillations, and the reason why they re distinct on the blende and feeble on the platinocyanide screen, that with the latter the sparks are seen on a luminous ground f general phosphorescence which renders the eye less able to see the cintillations.

Considering how coarse-grained the structure of matter must be to articles forming the emanations from radium, I cannot imagine that heir relative penetrative powers depend on difference of size. I ttribute the arrest of the scintillating particles to their electrical haracter, and to the ready way in which they are attracted by the oarser atoms or molecules of matter. I have shown that radium manations cohere to almost everything with which they come into ontact. Bismuth,* lead, platinum, thorium, uranium, elements of igh atomic weight and density, possess this attraction in a high egree, and only lose the emanations very slowly, giving rise to what known as "induced radio-activity." The emanations so absorbed

BAKERIAN LECTURE.—"On the Constitution of the Copper-Tin Series of Alloys." By C. T. HEYCOCK, F.R.S., and F. H. NEVILLE, F.R.S. Received February 26,—Lecture delivered February 26, 1903.

(Abstract.)

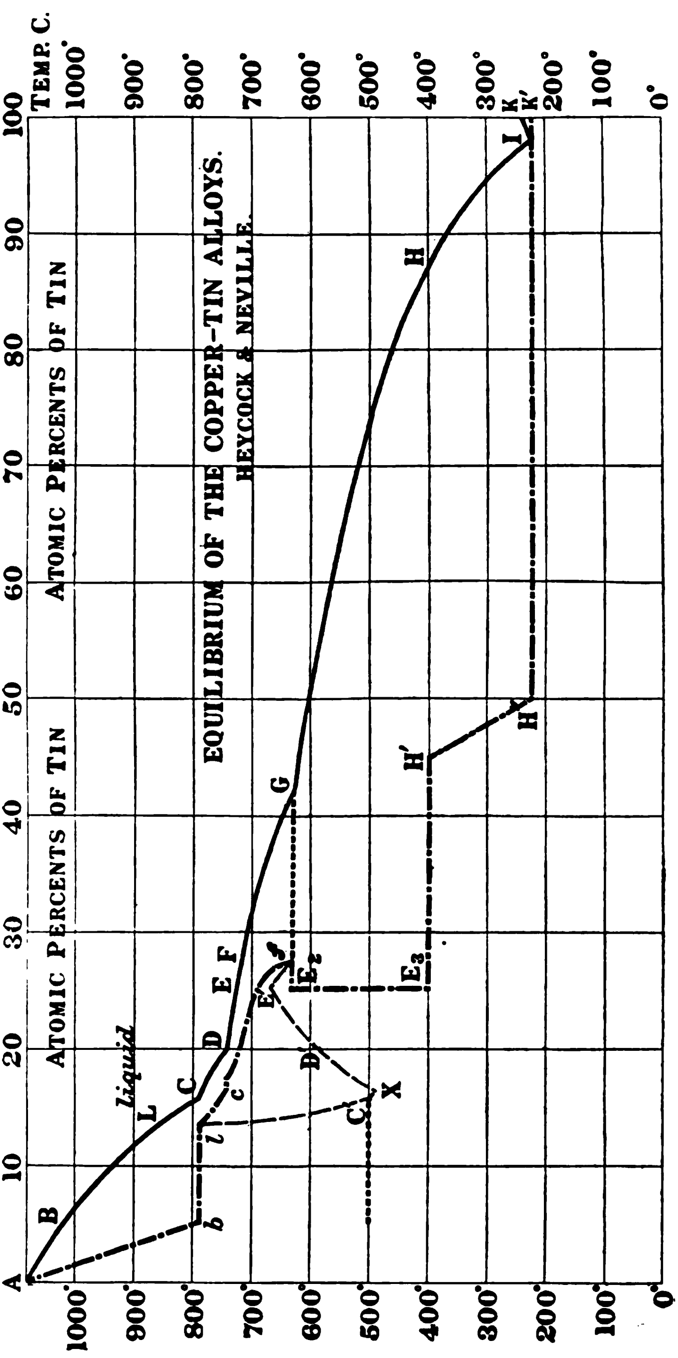
This paper is an attempt to fill a very serious gap in the study of alloys. As a rule, an alloy begins to be interesting when the temperature of the liquid alloy has fallen to its freezing point. This point, which records the moment when solid first appears in the liquid, is easily observed on account of the evolution of latent heat that occurs on the formation of solid, and if the freezing points of all the alloys of a series are determined, we can plot the freezing-point curve. Many such curves have been traced in the last ten years: that of the copper-tin alloys is given by the upper line in our diagram. The curve consists of several branches cutting each other in angular points. The one thing that these curves record without ambiguity is the number of different solids that can crystallise out of the liquid alloys, for each branch corresponds to the crystallisation of a different substance. But this is almost all that such curves tell us with certainty. They do not tell us whether the solids forming are the pure metals, or pure compounds, or crystalline solid solutions of the metals. Other experiments are needed to decide such questions.

The other great branch of the study of alloys consists in the microscopic examination of the solid alloys after they have cooled to ordinary temperature, that is to say, after they have, in general, ceased to undergo change. Between these two series of experiments there is an enormous gap of temperature, it may be 1000 or 500 degrees, and it is in this range of temperature that the whole life-history of the alloy, regarded as an organism, is to be found. The only fruitful experiments we know of dealing with this intermediate region are the cooling curves of Roberts-Austen and Stansfield. These observers traced automatically the whole of the cooling of the bronzes and obtained some remarkable results. They found that the evolution of heat at the freezing point was often succeeded at much lower temperatures by other evolutions of heat, and that many of these must have occurred after the alloy had wholly solidified. These thermal changes point to important chemical or physical changes, though they do not tell us what these changes are. They suggest, however, that the final patterns found by the microscope in the solid alloys are likely to be very complicated, as they may contain several records superposed the one on the other. We found these patterns very beautiful, but hopelessly complicated.

It occurred to us, and this is the method of the paper, that we could simplify the phenomena by a systematic chilling of the ingots at selected temperatures. A number of small ingots of the same alloy were placed in separate tubes in a bath of tin, together with a recording pyrometer, the temperature was raised above the freezing point of the alloy, and the whole very slowly cooled, the slow cooling being an essential feature of the experiment. Ingots were then extracted at selected temperatures and rapidly chilled by immersion in water. The microscopic examination of these chilled ingots showed that it was quite easy to distinguish the large crystals, that had formed during the slow cooling preceding the chill, from the matter that was liquid when the ingot was withdrawn from the furnace.

Successive chills of an alloy exhibit the solid growing in amount as the temperature falls, and finally show the ingot completely full of solid. We thus obtain, with very reasonable accuracy, the temperature of complete solidification of the alloy; and by applying the method to alloys with different percentages of tin we have traced a new curve, the "solidus," or curve of complete solidification. The solidus of the bronzes is the second line of the diagram. It is a remarkable line, made up of sloping, horizontal and vertical branches. As in the freezing-point curve, each branch corresponds to the crystallisation of a different solid.

In the notation of Professor Roozeboom the upper curve ABC


EQUILIBRIUM OF THE COPPER-TIN ALLOYS.
HEYCOCK & NEVILLE.
ATOMIC PERCENTS OF TIN
ATOMIC PERCENTS OF TIN
TEMP. C.
1000°
900°
800°
700°
600°
500°
400°
300°
200°
100°
0°
A
10
20
30
40
50
60
70
80
90
100
Liquid
A
B
b
L
l
C
c
C'
D
D'
E
E'
E2
E3
F
f
X
G
H
H
H'
I
K
K'

theory if time is allowed for them. The change at the G temperature is the breaking up of a solid solution into a mixture of the compound Cu_3Sn and liquid, and is instantaneous: here we have a case of a solid partially melting as it cools.

The curve lXE′*f* forms with the part of the solidus immediately above it an area, roughly triangular, within which all the alloys appear to be uniform solid solutions, but, as soon as an alloy cools to the curve, it becomes saturated and a new body crystallises out of the solid solution. One branch of the curve lXE′*f* corresponds to the crystallisation of a body rich in copper, the other to the crystallisation of a body rich in tin, which is probably the pure compound Cu_4Sn. The angle X (or rather C′), is the eutectic angle at which both bodies crystallise together, the whole phenomenon being exactly like crystallisation out of a liquid.

All the results obtained from the study of the chilled alloys are in harmony with the pyrometric work of Roberts-Austen and Stansfield, and many of the changes we have examined correspond to an evolution of heat recorded by them.

The paper is an extension of a short paper published by us in the 'Proceedings' of December, 1901.

Australia, Florida, Honduras, and the Bahamas, are underlaid by outliers of the adjoining land masses, which crop out as islands and islets on the very outer edge of the Barrier Reefs. Some of the Barrier Reefs of the Society Islands, of Fiji, and of the Carolines, show that the wide and deep lagoons, separating them from the land mass, have been formed by erosion, from a broad fringing reef flat. Encircling reefs, such as characterise especially the Society Islands, hold to their central island or islands the same relation which a Barrier Reef holds to the adjoining land mass. Denudation and submarine erosion fully account for the formation of platforms upon which coral reef and other limestone organisms may build, either barrier or encircling reefs, or even atolls, rising upon a volcanic base, of which the central mass may have disappeared, as in Fiji, the Society and Caroline Islands.

We may next take the type of elevated islands of the Paumotus, the Fiji, the Gilbert, and the Ladrones, many composed only of tertiary limestones, others partly of limestone, and partly volcanic. We can follow the changes from an elevated island, like Niue, or Makatea in the Paumotus, to an island like Niau, through a stage like Rangiroa to that of the great majority of the atolls in the Paumotus. The reef-flats and outer reefs flanking elevated islands, hold peculiar relation to them, they are partly those of Barrier Reef and partly of Fringing Reef. We may also trace the passage of elevated plateaux like Tonga, Guam, and islands in Fiji, partly volcanic and partly limestone, to atolls where only a small islet or a larger island of either limestone or volcanic rock is left to indicate its origin. Atolls may also be formed upon the denuded rim of a volcanic crater, as at Totoya and Thombia in Fiji, as well as in some of the volcanoes east of Tonga.

In the Ellice and Marshall group and the Line Islands, are a number of atolls, the underlying base of which is not known, and where we can only follow the formation of the land rim of the atoll, as far as it is due to the agency of the trades or of the monsoons in constantly shifting the superficial material (prepared by boring organisms) which goes to form its rim. Many of the atolls in the Pacific are merely shallow sinks, formed by high sandbanks, thrown up around a central area.

Throughout the Pacific, the Indian Ocean, and the West Indies the most positive evidence exists of a moderate, recent elevation of the coral reefs. This is shown by the bosses, pinnacles, and undermined masses of modern or tertiary limestone left to attest it. The existence of honeycombed pinnacles of limestone within the lagoons of atolls, as shoals, islands, or islets, shows the extent of the solvent action of the sea upon land areas, having formerly a greater extension than at the present day. Signs of this solvent action are to be seen everywhere among coral reefs. Atmospheric denudation has played an important part in reducing elevated limestone islands to the level of the sea, by riddling them with caverns and by forming extensive sinks, often taken to be elevated lagoons.

Closed atolls can hardly be said to exist; Niau in the Paumotus is the nearest approach to one, yet its shallow lagoon is fed by the sea through its porous ring. Sea water may pass freely into a lagoon at low tide over extensive shallow reef flats where there are no boat passages. The land area of an atoll is relatively small compared to that of the half submerged reef flats. This is specially the case in the Marshall Islands and the Maldives where land areas are reduced to a minimum.

The Maldivian plateau with its thousands of small atolls, rings, or lagoon reefs, rising from a depth varying from 20 to 30 fathoms, is overwhelming testimony that atolls may rise from a plateau of suitable depth, wherever and however it may have been formed and whatever may be its geological structure. On the Yucatan plateau similar conditions exist regarding the formation of atolls, only on a most limited scale.

The great coral reef regions are within the limits of the trades and monsoons and areas of elevation, with the exception of the Ellice and Marshall Islands and some of the Line islands. The extent of the elevation is shown by the terraces of the elevated islands of the

"The Electrical Conductivity imparted to a Vacuum by Hot Conductors."

By O. W. RICHARDSON, B.A., B.Sc., Fellow of Trinity College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received February 28,—Read March 26, 1903.

(Abstract.)

The experimental part of this paper is an investigation of the electrical conductivity of the space surrounding hot surfaces of platinum, carbon, and sodium, at low pressures. In addition, the first portion of the paper is occupied in deducing a theory by which the experimental results are explained. Some of the results that have been obtained with platinum were described in a preliminary note read before the Cambridge Philosophical Society on November 25, 1901.

The present paper is subdivided as follows:—

A. Theoretical investigation.
  1. Calculation of the saturation current.
  2. Equilibrium of corpuscles near a hot conducting plane of infinite area.

B. Experimental investigation.
  1. Experiments with platinum.
  2. Experiments with carbon.
  3. Experiments with sodium.

C. Conclusion.

The experiments show that the negative leak from hot wires at low pressures is a definite function of the temperature of the wire, and increases very rapidly as the temperature is raised. Professor McClelland* had previously found that this leak was independent of the pressure at pressures less than 0·04 mm., whilst Professor J. J. Thomson† had shown in addition that the current was carried by corpuscles or electrons.

The theory here put forward to explain these facts, and those to be described later, is based on the corpuscular theory of conduction in metals. On that view a metal contains a very great number of free corpuscles whose mean free path is comparable with that of a molecule in air at atmospheric pressure. The corpuscles must, therefore, be moving with a distribution of velocity given by the Boltzmann-Maxwell law. Since the corpuscles do not escape from the metal at ordinary temperatures, it is evident that there must be a discontinuity of potential at the surface which prevents their escape.

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* 'Camb. Phil. Soc. Proc.,' vol. 10, p. 241, and vol. 11, p. 296.

† 'Phil. Mag.,' vol. 48, p. 547.

On raising the temperature of the metal, the energy of the corpuscles is increased, and at high enough temperatures some of them will be able to shoot through the surface into the surrounding space.

The number of corpuscles which escape at any temperature has been calculated on this view. The saturation current, which corresponds to the number emitted per second, is given by the equation

$$C = n\epsilon S\left(\frac{R\theta}{2m\pi}\right)^{\frac{1}{2}} e^{-\Phi/R\theta},$$

where

n is the number of free corpuscles in 1 c.c. of the metal,
ϵ the charge on an ion,
S the area of the hot-metal surface,
R the gas constant for a single corpuscle, whose mass is m,
θ the absolute temperature, and
Φ the work done by an ion in passing through the surface layer.

The rate at which energy is emitted, due to this cause, is also calculated.

Owing to the important part which the ionisation from hot bodies has played in certain recent cosmological theories, the equations which determine the equilibrium of corpuscles near a plane surface of hot metal of infinite extent are also given and solved.

The chief problem which has been attacked experimentally is the for a considerable range of pressures. To account for the results at the higher pressures, it is necessary to assume that ions are produced by collisions.

The variation of current with temperature is examined over the following range:—

For platinum from 10^{-10} to 10^{-3} ampère per sq. cm. of surface.
For carbon „ 10^{-8} „ 2 „ „ „
For sodium „ 10^{-11} „ 2×10^{-2} ampère total current.

The corresponding ranges of temperature for platinum and sodium are roughly from 1000° C. to 1600° C., and from 100° C. to 450° C. respectively. The small currents from sodium were measured with a quadrant electrometer, but as a general rule, a sensitive D'Arsonval galvanometer with suitable shunts was used.

Perhaps the most striking result of the investigation is the relatively enormous currents which have been obtained. The biggest leak measured was 0·4 ampère from a carbon filament to an electrode placed near it; this corresponded to a current of 2 ampères per sq. cm. of the carbon surface, the potential on the filament being -60 volts. The pressure in this experiment was only $\frac{1}{600}$th mm. This current and some of the largest currents from sodium were registered on a Weston ammeter.

In all these experiments the potentials employed were too small to maintain a discharge between the electrodes.

Throughout the range given above, the relation between the saturation current and the temperature was found to be represented by a formula of the type

$$C = A\theta^{\frac{1}{2}}e^{-b/\theta},$$

where A and b are constants for each conductor.

The values which have been found for these constants are—

For platinum, $A = 10^{26}$, $b = 4\cdot93 \times 10^{4}$.
For carbon, $A = 10^{34}$, $b = \left.\begin{array}{r} 7\cdot8 \times 10^{4}. \\ 9\cdot7 \times 10^{4}. \\ 11\cdot9 \times 10^{4}. \end{array}\right\}$
For sodium, $A = 10^{31}$, $b = 3\cdot16 \times 10^{4}$.

The value of A varies very rapidly with the value found for b, so that only its order of magnitude is given.

The number n of free corpuscles in a c.c. of the metal is calculated from A. For platinum this gives $n = 10^{21}$, whereas Professor Patterson* found 10^{22}. In the case of the other conductors, the number found is absurdly great compared with Patterson's values.

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* 'Phil. Mag.' [6], vol. 3, p. 655.

The discrepancy can be made to disappear by assuming a small temperature variation of b. This assumption is shown to be consistent with the general nature of b.

The work required to drive an ion through the surface layer is calculated, in each case, from the value of b, to which it is proportional. Dividing by the charge on an ion this yields the discontinuity of potential at the surface of the conductor. The values found for this are: for sodium, $\delta\phi = 2{\cdot}45$ volts, for platinum $\delta\phi = 4{\cdot}1$ volts, and for carbon $\delta\phi = 6{\cdot}1$ volts. These numbers are inversely proportional to the cube roots of the respective atomic volumes. This leads to the conclusion that the work required to force a corpuscle out of a metal varies, approximately at any rate, inversely as the cube root of the atomic volume of the metal.

In all these experiments, the current when the hot wire is charged positively is small compared with that obtained with the metal negatively charged. Only in the case of sodium was the positive current large enough to deflect a sensitive galvanometer.

The results which have been obtained are shown to furnish a complete explanation of the phenomenon known as the Edison effect.

The fact that such enormous currents are obtained at such very low pressures confirms the conclusion that the ions are not produced from

"On a New Series of Lines in the Spectrum of Magnesium."

By A. FOWLER, A.R.C.Sc., F.R.A.S., Assistant Professor of Physics, Royal College of Science, South Kensington. Communicated by H. L. CALLENDAR, F.R.S. Received March 9,—Read March 26, 1903.

Although the spectrum of magnesium has been the subject of many investigations, certain lines which occur in the arc spectrum appear to have hitherto escaped notice. The lines in question are comparatively feeble, but on account of their theoretical interest it seems desirable to draw attention to them.

The new lines make their appearance in the spectrum when the arc is made to pass between poles consisting of magnesium rods, but they do not always appear with equal intensity. They are somewhat nebulous, especially on their less refrangible sides, so that their positions cannot be determined with great accuracy; but as nearly as they can be ascertained with the instruments at my disposal, the wave-lengths are (in air), 4511·4, 4251·0, 4106·8, and 4018·3.

A mere inspection of the photographs suggests that these lines constitute a regular series, associated with the much stronger series described by Rydberg*, having wave-lengths 5528·75, 4703·33, 4352·18, 4167·81, 4058·45, and 3987·08, according to the measures of Kayser and Runge. This view seems to be sufficiently confirmed by calculation.

Rydberg found that neither his own general formula nor that of Kayser and Runge could be applied with sufficient accuracy to the stronger series, and employed a combination of the two formulæ, namely,

$$n = a + \frac{b}{(m+\mu)^2} + \frac{c}{(m+\mu)^4},$$

where n is the oscillation frequency, m has the values 3, 4, 5, &c., and a, b, c, and μ are constants to be determined from four lines belonging to the series. For the magnesium series, the equation calculated by Rydberg is

$$n = 26{,}631{\cdot}44 - \frac{111{,}856{\cdot}92}{(m+0{\cdot}406)^2} + \frac{147{,}764{\cdot}05}{(m+0{\cdot}406)^4},$$

n being the oscillation frequency *in air*, and m having the values 3, 4, 5, 6, 7, 8 for the six lines named above.

Using the same formula for the new series, and calculating the constants from the four lines, the equation for frequencies *in vacuo* is

$$n = 26{,}595{\cdot}4 - \frac{102{,}496{\cdot}6}{(m+0{\cdot}618)^2} + \frac{168{,}840{\cdot}5}{(m+0{\cdot}618)^4}.$$

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* 'Öfversigt af Kongl. Vet. Akad. Forhandl.,' 1893, Stockholm.

Another formula* which may be conveniently employed is

$$n = n_\infty - \frac{C}{(m+\mu)^2 - m_0}.$$

This formula gives for the two magnesium series the equations:

"Rydberg" series, $n = 26{,}601{\cdot}49 - \dfrac{107{,}071{\cdot}37}{(m+1{\cdot}2304)^2 + 2{\cdot}13282}$,

New series, $n = 26{,}587{\cdot}4 - \dfrac{100{,}033{\cdot}6}{(m+0{\cdot}495)^2 + 2{\cdot}38919}$,

n being the oscillation frequency *in vacuo* in each case.

It will be seen that the convergence frequency of the new series is as nearly equal to that of the Rydberg series as can be expected with the comparatively rough wave-lengths employed, and it may be added that in each case the constant m_0 is of unusual magnitude. These facts, in conjunction with the general characters and relative intensities of the lines, render it highly probable that the new series is associated with the Rydberg series as second and first subordinate series respectively.

Applying the formula to the calculation of the members of the new series for which $m = 3$ and $m = 2$, the corresponding wave-lengths in air are 5065·0 and 6674·5. The first is probably represented by a line having an approximate wave-length 5067, which is not so readily observed in the photographs as the others, because the plates employed are comparatively slow for this part of the spectrum, and if the exposure be lengthened, the banded spectrum of magnesium becomes strong enough to almost mask the line. The line 6674·5 is perhaps too far in the red to be conveniently observed, seeing that it is probably feeble and not well defined.

It may be reasonably concluded that the arc spectrum of magnesium includes two subordinate series of single lines in addition to the two well-known subordinate series of triplets. No such combination of series appears to have been previously noted in the spectrum of a metal, but two sets of series, each set consisting of a principal and two subordinate series, are well-known in the spectra of helium and oxygen.

The author desires to express his obligations to Mr. Herbert Shaw for valuable assistance in making the computations involved in investigating the various formulæ which have been suggested for series, as applied to the series which forms the subject of the present paper.

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* After much labour, this formula was arrived at by Mr. Herbert Shaw and the author as the one giving the most consistent results for series in general, but it was afterwards found that a similar formula, expressed in wave-lengths, had [illegible]y been employed by Mr. Rummel ('Roy. Soc. Victoria Proc.,' vol. 10, Part I, 75).

"An Attempt to Estimate the Relative Amounts of Krypton and of Xenon in Atmospheric Air." By Sir WILLIAM RAMSAY, K.C.B., F.R.S. Received March 9,—Read March 26, 1903.

When Dr. Travers and I isolated krypton and xenon from air, we had very little idea of the total amount of liquid air from which, by its evaporation, these gases had been obtained. And we were then more concerned with the isolation of the gases in a pure state than in the determination of the proportion in which they exist in the atmosphere. Our knowledge of the composition of the air, however, is not complete until the total yield of krypton, xenon, neon, and helium has been determined. An estimation of the two last is being undertaken by Dr. Travers.

In our experiments on these gases* we did not measure the total quantity of air evaporated. We used liquid air for various purposes, and for some months we collected the dregs, allowing them to evaporate into a large gas-holder. We guessed (but it was only the roughest estimate) that we had accumulated in this manner the residues from about thirty litres of liquid air; and on this assumption we thought the following estimates not improbable:—Helium, 1—2 parts per million of gaseous air; neon, 1 per 100,000; krypton, 1 per million; xenon, 1 per 20,000,000. But they rested on a very insecure foundation of fact.

The first preliminary experiment was made to ascertain how much of the air which passes through the Hampson liquefier is converted into liquid. The results, however, were inaccurate, and I would rather cite the conclusions given by later experiments on a much larger scale. The compressor was run for several hours every morning and afternoon during six and a half days; the liquefied air was weighed after each run; and the escaping air passed through a large gas-meter, where its volume was registered. The air escaping had a somewhat lower density than ordinary air, owing to the partial removal of oxygen and argon; but the experiments were not sufficiently accurate to make it worth while to take this into account. The volumes given are, however, corrected for alterations of pressure and temperature. In all, 179·7 kilos. of gaseous air passed the meter, and 10·8 kilos. of liquid air were collected. During the collection, about 6 per cent. of the liquid air evaporates; adding this, 11·4 kilos. must have been the total weight of air liquefied. The total weight of air taken in by the compressor, therefore, and delivered to the liquefier was consequently 179·7 + 11·4 = 191·1 kilos, and the percentage liquefied a little under 6 per cent. The number may be taken without sensible error as 6 per cent. liquefaction.

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* "Argon and its Companions," 'Phil. Trans.,' A, vol. 197, pp. 47—89.

During these runs the pressure was kept at 190 atmospheres; and the delivery of air through the escape valve of the liquefier remained fairly uniform. Separate readings, taken at different times, of the amount of air passing the meter gave 0·244, 0·230, 0·258, 0·261, 0·248, and 0·239 kilos. per minute; these figures testify to fairly uniform working.

It was of interest to see whether argon concentrated itself in the liquefied portion of the air, or whether most of it escaped as gas. The boiling points of the principal atmospheric gases are, on the absolute scale:—nitrogen, 77°·54; argon, 86°·90; oxygen, 90°·5; the differences are 9°·36 and 3°·6 respectively; it was therefore to be expected that the argon would concentrate in the liquid.

An experiment was therefore made in which 12·73 grammes of freshly collected liquid air was allowed to boil in a double-walled vacuum-tube, and the gases were led directly over metallic copper and magnesium lime; the resulting argon measured 165 c.c. at 0° and 760 mm. pressure. The weight of this argon was 0·2943 gramme, or from 1 gramme of liquid air, 0·02312 gramme. Now 1 gramme of atmospheric air contains 0·0129 gramme of argon; hence the process of liquefaction nearly doubles the content of argon in the air. It is, therefore, very advantageous to prepare argon from air which has been liquefied.

another hole in the cork there passed a siphon which could be closed by means of a brass stop-cock; this tube served to admit liquid air to the balloon. A manometer was also connected with the interior of the flask so as to register the pressure under which the liquid air was boiling. The air boiled at a pressure of about 250 mm., corresponding to a temperature of about - 195°. The boiling was quite quiet, without bumping; it was sometimes necessary to warm the balloon gently in order to accelerate the evaporation. The object of distilling at a low temperature was to lower the vapour-pressure of the krypton and xenon in the liquid air, and so to lessen, or in great part to prevent their evaporation. The total liquid air was thus reduced to about 200 c.c.

The balloon containing this air was coupled with a large iron tube, holding about 20 kilos. of reduced copper heated to bright redness. The liquid air residue, naturally, consisted largely of oxygen, for the more volatile nitrogen had in great part evaporated. After passing over the copper the volume of gas was about 50 litres.

It may be contended that during the evaporation of the air, even at - 195°, a large portion of the krypton and xenon may pass away as gas. It is not possible to estimate the amount lost in this manner; but at - 195°, the vapour pressure of krypton is 2·8 mm., and that of xenon 0·02 mm. These figures have been arrived at in the following manner. Relying on the vapour pressures of mercury by Ramsay and Young, given in the *Trans. Chem. Soc.*, 1886, p. 50, and on the measurements made by Ramsay and Travers* of the vapour pressures of krypton and xenon, ratios were found between the absolute temperatures of mercury on the one hand, and of krypton and xenon respectively on the other, between the pressures 300 and 3000 mm., with two additional data—the temperatures of krypton corresponding to pressures of 9 and of 17·4 mm. They are as follows:—

Pressures.	Temp. of mercury. Degrees absolute.	Temp. of krypton. Degrees absolute.	Ratios.	Temp. of xenon. Degrees absolute.	Ratios.
mm.					
9·0	454°·8	94°·2	0·1851	—°	—
300	582·2	110·4	0·1897	148·9	0·2257
400	596·4	113·8	0·1909	153·2	0·2569
500	609·0	116·1	0·1907	156·8	0·2575
600	617·9	118·35	0·1915	159·7	0·2583
700	626·5	120·2	0·1918	162·0	0·2586
760	631·2	121·3	0·1922	163·9	0·2597
3000	726·8	142·2	0·1957	192·4	0·2647

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* 'Phil. Trans.,' A, vol. 197, 1901, p. 72.

These ratios were mapped against the absolute temperatures of iercury, and were found, as usual, to give straight lines. The lines vere extrapolated to lower temperatures, and the vapour pressures equired were calculated from the extrapolated curves.

The portion of interest is given in the following table:—

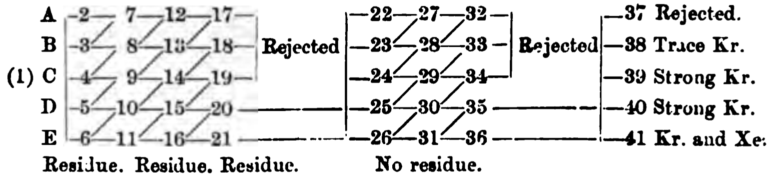
Temp. C.	Vap. press. of krypton.	Vap. press. of xenon.
– 205°	0·27 mm.	0·0005 mm.
– 200	0·97 „	0·007 „
– 195	2·8 „	0·02 „
– 190	7·5 „	0·04 „
– 188·8	9·0 „	0·11 „
– 182·4	17·4 „	0·17 „

The last two data for krypton are the results of direct measurenent. It may be mentioned here that the melting point of krypton s about – 169°, and that of xenon – 140°; and the boiling points at tmospheric pressure are, krypton – 151°·7, and xenon – 109°·1.

The nitrogen was removed from the 50 litres of gas by passing it ver red-hot magnesium-lime mixture. The resulting crude argon neasured 12·5 litres at 16° and 770 mm.; its weight is calculated from ts known density as 21·3 grammes.

This argon was liquefied in a bulb immersed in liquid air boiling

Residue of 1500 c.c.:—



No. 41 was mixed with the residues, which contained much xenon, along with krypton. Nos. 38, 39, and 40 were mixed, and the fractionation continued in a small apparatus. The gases were now sparked with oxygen over soda, so as to remove traces of air; for the operations had now to be conducted with great care, and the spectra of the two samples showed traces of nitrogen. After withdrawal of oxygen from both sets of gas, by means of phosphorus, the krypton mixture was fractionated into three portions; 42, containing much argon; 43, rich in krypton; and 44, a non-volatile residue. The xenon residues, 41, were also solidified with liquid air, and placed for a few seconds in connection with the vacuum of the Töpler pump. The gas which was pumped off was added to 43, and the residue to 44. The bulb was now left for a quarter of an hour, so that equilibrium might be restored, and the stop-cock opened a second time. A bubble or two was removed with the pump. On making communication with the pump a third time, no gas escaped; and this is not remarkable, for the vapour pressure of xenon is little over 0·1 mm. at that temperature. It was therefore assumed that the final residue was pure xenon; and, indeed, its spectrum showed no trace of the krypton lines. And it may also be taken for granted that very little xenon was added to No. 43. The densities of Nos. 42 and 43 were determined; and from the known densities of argon and krypton, their relative proportions were calculated.

The density of 42 was found to be 21·31, corresponding to the percentage composition, argon, 93·5 per cent., krypton, 6·5 per cent.; the density of 43 was 39·43, implying a mixture of argon, 6·6 per cent., krypton, 93·4 per cent. The volume of No. 42, reduced to 0° and 760 was 22·0 c.c.; it therefore contained 1·43 c.c. of krypton; that of No. 43 was 6·5 c.c., and it contained 6·1 c.c. of krypton. The total volume of krypton was therefore 7·5 c.c., and its weight 0·0028 gramme; the volume of the xenon, reduced to 0° and 760 was 0·87 c.c., and its calculated weight, 0·0005 gramme.

These results are reproduced in the following tabular statement:—

Air passed through liquefier...............	191·1 kilogrammes.	
Air liquefied	11·3 „	= 5·91 p.c.
Argon obtained (11·8 litres at 0° & 760)	21·3 grammes.	
„ per cent. of gaseous air............	0·0118; of liquid air, 0·1885.	

Total krypton obtained......................	0·0028 gramme.
,, xenon obtained	0·0005 ,,
Percentage krypton in gaseous air	0·000014 by weight.
,, xenon ,, ,, ...	0·0000026 ,,

Krypton equal to 1 part by weight in about 7 millions of air; by volume, 1 part in 20 millions.

Xenon equal to 1 part by weight in about 40 millions of air; by volume, 1 part in 170 millions.

As before remarked, it is not maintained that all the krypton and all the xenon have been separated; it is likely, however, that the separation of the xenon was more perfect than that of the krypton. The results are merely brought forward as the result of a careful experiment to quantitatively isolate these gases.

I have to express my cordial thanks to Mr. E. C. C. Baly and to Mr. Inglis for aid in carrying out part of these operations.

As a quantity of pure krypton, sufficient for determination of density, had been collected, occasion was taken to redetermine the density of that gas. It was submitted to careful fractionation; a considerable portion was rejected as possibly containing argon, and the dregs were set aside as possibly having contained xenon. The substance weighed had a low vapour pressure,—about 15 mm. at the

"Some Physical Properties of Nickel Carbonyl." By JAMES DEWAR, M.A., Sc.D., LL.D., F.R.S., Jacksonian Professor in the University of Cambridge, and HUMPHREY OWEN JONES, M.A., Fellow of Clare College, Jacksonian Demonstrator in the University of Cambridge. Received March 3,—Read March 26, 1903.

The properties of nickel carbonyl have until recently been the subject of but few investigations. Dr. Mond and his collaborators in the discovery of this remarkable substance determined some of its physical properties, including its boiling point, specific gravity, and vapour density. Subsequently Dr. Mond, in association with Professor Nasini, made observations on its molecular refraction and thermal expansion.

A substance of the peculiar molecular structure of nickel carbonyl seemed to call for further study. The investigation described in the present paper was carried out in the winter of 1901; the authors' intention being to make a complete study of the stability of the compound both in the gaseous and liquid conditions. While the work was in progress a paper by Mittasch was published* containing an account of an admirable and exhaustive investigation of the velocity of the reaction between nickel and carbon monoxide, including the heat of formation and vapour tensions of the compound, covering part of the ground which we had examined. A number of interesting problems examined in the course of our inquiry however remain, which have not been touched upon by previous investigators, and to some of these the present paper is devoted.

The vapour density of nickel carbonyl was determined by Mond, Langer and Quincke† in air, at 50° C. by Victor Meyer's method. The value obtained was 86·7, while theory requires 85. The vapour density at this temperature is quite normal, and there is no evidence of association even at this temperature only some 7° C. higher than the boiling point of the compound. It was found that the vapour exploded at 60° C. with a flash of light, and carbon dioxide was detected among the products of decomposition. Berthelot‡ explained the explosion as being due to the production of carbon dioxide by the intermolecular reaction:—

$2CO = CO_2 + C$, which was observed to take place when carbon monoxide acts on nickel at 350°—450° C. Later§ it was found that the action only proceeded in this way to a small extent, when nickel carbonyl vapour was decomposed. The reaction $2CO = CO_2 + C$ liberates 38·8 kilogramme calories, so that if the heat of formation of nickel carbonyl is less than 77·6 kilogramme calories per molecule, this explanation would be valid. This condition is satisfied since Mittasch* has shown that the heat of formation of nickel carbonyl is between 50·6 and 55·6 cals. Hence the detonation observed by Mond might result from the reaction:

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* 'Zeit. Phys. Chem.,' 1902, vol. 40, p. 1.

† 'Jour. Chem. Soc.,' 1890, vol. 57, p. 749.

‡ 'Compt. Rend.,' vol. 112, p. 1343.

§ 'Ann. Chim. Phys.,' 1901 [7], vol. 22, p. 304.

$$Ni(CO)_4 = Ni + 2CO_2 + 2C.$$

The fact that nickel carbonyl was thus reported to be explosive, together with the explanation of its explosibility offered by Berthelot, strengthened the belief in its great instability and deterred experimenters from working with it, in any case at temperatures above its boiling point.

The authors observed that when nickel carbonyl was suddenly heated in an atmosphere of some inert gas, such as hydrogen or nitrogen, the vapour decomposed quietly with deposition of metallic nickel; there was no explosion or flash of light, and the quantity of carbon dioxide produced was so small, in most cases, as to be almost negligible. This was found to be the case even when the temperature used was as high as 130° C. Mittasch* states that he could investigate the effect of the gaseous product of dissociation on the stability of the carbonyl.

It was found that the rate of admixture of the vapour with the gases had a marked effect on the dissociation, as shown by the difference in the vapour densities, when taken under similar conditions, in the various inert gases; and, further, that the presence of carbon monoxide produced the expected diminution in the amount of the compound decomposed. In order to further confirm this, two reservoir tubes of different bore were used, one having a cross-sectional area about three times that of the other (the latter will be referred to as the narrow tube). In the latter, admixture could take place much less readily than in the former; consequently the surrounding gas would be expected to have a smaller effect on the extent of the dissociation. This was also confirmed by the experimental results; the vapour density in the narrow tube is almost independent of the gas employed.

The effect of the nature of the surface on the extent of dissociation was tested by using the tubes coated internally with a film of metallic nickel deposited from the vapour of nickel carbonyl by heating. The film of nickel seemed to bring about a state of equilibrium more rapidly, so that the vapour densities determined in these tubes were lower than those in the same tubes not covered by nickel (a similar effect was observed by Mittasch).

It was found that the rate at which the liquid evaporated, as would be expected in the case of a substance which readily dissociates, had some effect on the extent of the dissociation. Hence, it was necessary, in order to get comparable results, to arrange that approximately the same time was taken for vaporisation in all the experiments made at the same temperature. A definite end-point could be observed in each case at which the gas displaced by the vapour ceased to come off, and a much slower evolution of gas took place. The experiment was stopped when the more rapid evolution of gas gave place to the slower.

The results obtained are appended in the following table (Table I).

In the fourth column the percentages of nickel-carbonyl molecules dissociated are given, calculated from the formula $P = \frac{85 - D}{3D} \times 100$, where 85 is the theoretical vapour density of nickel carbonyl and D is the observed value.

Unless otherwise stated, it is to be understood that the determination was made in a Victor Meyer's apparatus of the usual type, occasionally referred to as the wide tube.

From the figures in the above table, it is seen that the value of the vapour density, deduced from the experiments in the wide tube, is greater in ethylene than in nitrogen and hydrogen. It was also

Table I.—Vapour Densities determined by Meyer's Method.

Temperature of the bulb. °C.	Gas filling the bulb.	Vapour density found (H = 1).	Percentage of $Ni(CO)_4$ dissociated.	Remarks.
63	Nitrogen	83·3	0·7	Very slight deposit of nickel.
		78·2	2·7	Slight deposit of nickel.
64	Hydrogen	79·2		
66	Carbon monoxide	83·9	0·15	Nickel covered tube.
		85·2		
81 (Benzene)	Nitrogen	71·0	6·2	Slight deposit of nickel.
		72·1		
100	Hydrogen	67·3	8·8	Nickel deposited over several inches in the tube.
		67·0		
	"	56·9	16·7	Wide tube covered with nickel.
		56·7		
	"	79·0	2·4	Narrow tube. Nickel deposit extended about 32 cm.
		79·8		
"	Nitrogen	70·8	6·7	
		70·9	..	Wide tube.
	"	75·6	4·1	Narrow tube.
"	Ethylene	70·6	6·8	Wide tube. Nickel deposited over 2 cm. from bottom of tube.
		70·7		
		73·7	4·3	Wide tube. Moist nickel carbonyl. Nickel deposited irregularly over about 3 cm.
		76·7		

noticed that the deposit of nickel in the latter two gases extended higher up the tube than in the former, and was higher in hydrogen than in nitrogen. In carbon monoxide, on the other hand, the vapour density is higher than in the gases hydrogen, nitrogen, or ethylene, the values at 100° C. being nearly normal, and the dissociation was incomplete even in aniline vapour. This demonstrates very clearly the effect of the presence of one of the dissociation products on the amount of the dissociation.

In the narrow tube, however, the values obtained at 100° C. do not seem to depend, to any great extent, on the nature of the surrounding gas, the values in carbon monoxide and in the inert gases being almost identical, which shows the great effect of rate of admixture and diffusion on the dissociation.

The amount of dissociation increases rapidly with the temperature; in nitrogen at 155° C. it is practically complete. The rate of increase in carbon monoxide is distinctly slower, the difference between the vapour densities in nitrogen and carbon monoxide at 129° C. being quite marked. Above 155° C. the results obtained are somewhat irregular; but dissociation seems to be nearly complete at atmospheric pressure, since only a small deposit of nickel could be obtained when the tube was placed horizontally and a clear part heated with the blowpipe.

A few vapour-density determinations were also made by Hoffmann's method at temperatures between 17° C. and the boiling point of aniline (182° C.) in order to observe the dissociation of the undiluted vapour. Complete dissociation is practically reached at 182° C., but even then the application of a pointed flame to a clear portion of the tube produced a slight deposit of nickel, so that traces of nickel carbonyl were still present.

The results are given in the appended table (Table II).

The results of the experiments in the narrow tube are given in the fifth column for the sake of comparison, the phenomena in this case being practically the dissociation of the vapour in contact with its own dissociation products. The dissociation is clearly greater under reduced pressure, as might be anticipated.

The general results of the vapour-density determinations are readily seen from the curves in fig. 1 (p. 433).

Having thus found that the vapour of nickel carbonyl was much more stable at elevated temperatures than had hitherto been suspected, we resolved to examine the stability of the liquid under pressure, and if possible make observations on it as far as its critical point.

Small sealed tubes, from one-half to one-third full of nickel carbonyl, were heated to 200° C. without bursting. A small quantity of nickel was deposited on the first heating, and it was found that its quantity was not appreciably increased on repeating the operation. On standing a sufficient length of time at the ordinary temperature the nickel gradually dissolved again, although it was covered by the excess of liquid. This proved in an interesting manner the ease with which the reaction between the deposited nickel and carbon monoxide is reversible.

The amount of the nickel deposition was notably greater when the liquid was heated in contact with mercury, so that the use of the Cailletet pump for the examination of the critical phenomena was not practicable.

Rough measurements of the critical temperature were made by observing the temperature at which the meniscus disappeared and

Table II.—Vapour Densities of Nickel Carbonyl by Hofmann's Method.

Temperature of bath.	Vapour density.	Pressure in mm. of mercury.	Percentage of nickel carbonyl dissociated.	Percentage dissociated at atmospheric pressure.	
				In nitrogen.	In carbon monoxide.
°C.					

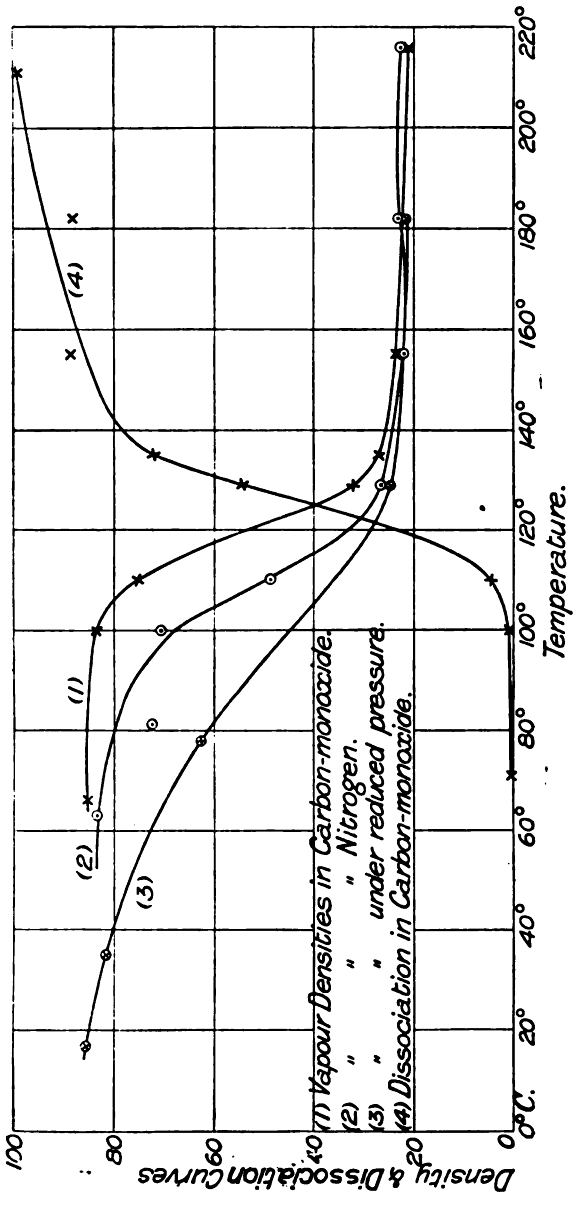

Fig. 1.

The presence of carbon monoxide would be expected to cause a lowering of the temperature at which the meniscus disappeared, and this expectation was realised in the course of the experiments. Hence the temperature observed in the first experiment is probably below the true critical temperature. The presence of 4—5 per cent. of carbon monoxide with its low critical temperature 128° abs. would be expected to lower the temperature at which the meniscus disappears by about 5° C. That this quantity of carbon monoxide might be present at any time is shown by the following experiment:—

Volume of tube, 1·35 c.c.
,, nickel carbonyl left after heating to 193° C. = 0·437 c.c.
Amount of nickel deposited = 0·0147 gramme.
,, carbon monoxide liberated = 0·0284 gramme = 5·02 per cent. of the residual nickel carbonyl.

Hence it is very probable that the true critical temperature is about 200° C.

Comparative experiments made with pure ethyl ether in similar tubes, gave a critical temperature of 193° C. The accepted value for the critical temperature for ether being 194°·5 C., the observed temperature for nickel carbonyl cannot be far removed from the correct value.

The formula $T_b = 0{\cdot}66\ T_c$, where T_b is the absolute boiling point and T_c the absolute critical temperature, should be applicable to the case of nickel carbonyl since it is applicable to ether, a liquid which has a critical temperature of approximately the same value. Taking the boiling point to be 43°·3 C. the critical temperature calculated temperature, so that the pressure in the tube seems to be rather more than 40 atmospheres at the critical point. On cooling, the indicating globule remained permanently displaced some distance up the tube, showing the existence of a pressure developed by the decomposition of the nickel carbonyl. On standing for some time the whole of the nickel disappears, and the carbonic oxide pressure disappears.

The pressure on cooling seemed to be about ten atmospheres, hence the critical pressure would be about thirty atmospheres. Later it will be shown that this is near the actual value.

The critical constants of the compound being known, together with the boiling point, it is possible to calculate a vapour-pressure curve. It was, however, thought better to determine the vapour pressure at a number of temperatures below the boiling point of the liquid, by the static method, and from this curve by extrapolations to deduce the values for higher temperatures.

A wide barometer tube (about 0·7 cm. diameter) was carefully cleaned, dried, and drawn off to a fine capillary tube at one end. The tube was then placed upright in a vessel of pure dry mercury and exhausted thoroughly with a Fleuss pump. A small tube full of nickel carbonyl was now introduced at the bottom of the tube and the whole then exhausted again, while surrounded by a freezing mixture, in order to get rid of all adhering air, and finally sealed off rapidly at the fine capillary. By this method of procedure only a very small amount of decomposition took place during the sealing off, as indicated by the very slight deposit of nickel. The pressure was then read off by means of a kathetometer, while the tube was surrounded by a bath kept at various constant temperatures.

The results are appended below, together with those obtained by Mittasch* by the dynamic method.

Dewar and Jones.		Mittasch.	
− 9° C.	94·3 mm.	2°·05 C.	133·1 mm.
− 7	104·3	7 ·5	170·5
− 2	129·1	15 ·27	238·2
0	144·5	20 ·2	294·3
+ 10	215·0	24 ·26	349·7
+ 16	283·5	29 ·52	444·2
+ 20	329·5	34 ·29	532·6
+ 30	461·9	39 ·97	647·2

The values for − 9° C. and + 30° C. give the following Rankine formula for the relation between the pressure p in millimetres of mercury and the absolute temperature T. $\log p = 7{\cdot}355 - 1415/T$. At 200° C. (about the critical temperature) the pressure calculated from this equation is 30·4 atmospheres. Taking the results obtained by Mond and Nasini, viz., the boiling point 43° C. 751 mm. and the pressure at 20° C. 338·7 mm., a similar expression, log $p = 7{\cdot}281 - 1392/T$, is obtained.

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* *Loc. cit.*

Taking Mittasch's pressures for 2° C. and 40° C. the Rankine obtained is :—

$$\log p = 7{\cdot}781 - 1555{\cdot}7/T.$$

The boiling points calculated from these formulæ are given below :—

Mond........................	43°·3 C.
Mittasch	44°·4
Dewar and Jones	43°·2

In order to further confirm our vapour-pressure determinations, we made a determination of the boiling point of some carefully dried and redistilled nickel carbonyl. With the barometer at 769 mm. the liquid boiled at 43°·2 – 43°·33 C.

It would therefore appear that our vapour tension curve is the more accurate. The following curves, fig. 2, illustrate very clearly the extent of the deviations at different temperatures.

The latent heat of vaporisation of $Ni(CO)_4$, is 38·1 calories per gramme, and the Trouton constant (molecular latent heat divided by the absolute boiling point) is 20·6, its value for ether being 22. The number obtained by dividing the absolute critical temperature by the critical pressure, which is proportional to the volume of the molecule, Van der Waal's constant *b*, is 15·5; the similar number for carbon monoxide is 3·7, so that nickel carbonyl according to theory has a molecule 4·2 times larger than carbon monoxide. The molecular volume of nickel carbonyl at its boiling point is 136, as compared with 110 for ether. The critical density appears to be about 0·46, while that of ether is only 0·25. If the liquid densities of Mond and Nasini* are taken with the critical data, then the Waterston formula $v = 2{\cdot}0398 - 0{\cdot}5667 \log (198{\cdot}7 - t)$ fits in very well with the results. A similar formula for ether given by Avenarius is:—

$$v = 3{\cdot}19 - 0{\cdot}802 \log (191 - t).$$

The molecular volume of nickel carbonyl at its boiling point is 136, subtracting from this 7·2 for the nickel atom, we have 32·2 as the volume of each molecule of carbon monoxide in the molecule. Now liquid carbon monoxide at its boiling point has the molecular volume of 35, so that contraction would take place if liquid carbon monoxide could combine with nickel. The heat of formation of nickel carbonyl is about four times greater than that of the liquefaction of the equivalent amount of carbonic oxide under normal conditions.

The experiments described above show clearly that nickel carbonyl is a substance admirably suited for the demonstration of the phenomena of dissociation. Great care must, however, be taken in handling the substance, owing to its poisonous properties when inhaled. It also forms an excellent illustration of a reversible reaction, and the following experiments serve to illustrate the way in which it may be used for this purpose.

A number of carefully dried tubes were exhausted by means of a Fleuss pump, and were then filled with a mixture of 10 per cent. nickel carbonyl vapour and 90 per cent. carbon monoxide, at pressures of 50, 100, 226, 304, 396, 504, and 624 mm. of mercury respectively. These tubes were then heated in a bath until nickel began to deposit; the tube under observation was then kept at that or a slightly higher temperature for about half-an-hour, and afterwards tested for the presence of nickel carbonyl by heating a clean portion of the tube with a fine pointed flame, and in this way the presence of even a very small trace of nickel carbonyl was immediately detected by the formation of a bright mirror of nickel on the hot part of the tube. The tube at 50 mm. pressure did not deposit nickel when placed in alcohol vapour, ut did so at 100° C., and, after heating for a few hours at this temperature, no nickel carbonyl could be detected in the tube. After tanding for a few days, however, nickel was deposited when a clean ortion of the tube was heated with a pointed flame, thus showing hat nickel carbonyl had been regenerated. In the 100 mm. tube ickel was still deposited after heating for two days at 100° C.; all he nickel carbonyl was found to have disappeared after the tube had een heated for some time to 130° C. Nickel was deposited in the ube at 226 mm. pressure at 130° C., in the 301 mm. tube at 158° C., nd in the other tubes at slightly higher temperatures. In the two ubes at the highest pressures there was a considerable quantity of he carbonyl present after heating for an hour at 160° C. All the ubes in which the nickel carbonyl had been so far destroyed that no isible deposit of nickel could be obtained on heating a clean portion f the tube with a small flame, after standing for a few days ontained enough of the carbonyl to be readily detected by the bove test.

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* *Loc. cit.*

Another form of experiment suitable for demonstration proved the reaction proceeded rapidly at the ordinary temperature, and with a measurable velocity at low temperatures, even when the pressure of the carbonic oxide atmosphere was below 200 mm. A large bulb of about 200 c.c. capacity was connected to a mercury

The volatile iron carbonyl has been made the subject of a number of similar observations, dealing with its physical properties and chemical stability, which will be discussed in another communication.

"An Enquiry into the Variation of Angles observed in Crystals, especially of Potassium-Alum and Ammonium-Alum." By Professor H. A MIERS, M.A., D.Sc., F.R.S. Received March 10,—Read March 26, 1903.

(Abstract.)

Corresponding angles measured on different crystals of the same substance usually differ slightly. On cubic crystals the theoretical angles are known. Pfaff professed to have established that only those cubic crystals which display birefringence exhibit divergence from the theoretical angles, but Brauns showed that in lead nitrate, ammonia-alum, and spinel, for both isotropic and birefringent crystals alike, the octahedron angle may differ by as much as 20′ from that of the regular octahedron.

The author has endeavoured to trace the changes of angle upon one and the same crystal during its growth by measuring it at intervals without moving it from the solution in which it is growing. This is accomplished by means of a new telescope-goniometer in which the crystal is observed through one side of a rectangular glass trough, and the changes in the inclination of each face are followed by watching the displacements of the image of a collimator slit viewed by reflection in it. The crystal is held by a platinum clip which it envelopes as it grows. Small movements of the image are followed by means of a special micrometer-eyepiece which accurately measures the magnitude and direction of the displacement.

Examined in this way an octahedron of alum (ammonium or potassium) is found to yield not one but three images from each face; and closer inspection shows that the crystal is not really an octahedron, but has the form of a very flat triakis octahedron. It often happens that of the three faces which nearly coincide, one is large and the remaining two very small, so that of the three images one is bright and the others are very faint, and can only be discerned with difficulty; in such a case the crystal as measured in the ordinary way would appear to be an octahedron whose angle differs from the theoretical value by a few minutes.

When a growing crystal of alum is watched for several hours or days, it is found that the three images yielded by an apparent octahedron face continually change their position; one set fades away and is replaced by another set, which are generally more widely separated than those which they succeed. The images move in three directions inclined at 120° to each other, and indicate that these faces always belong to triakis octahedra. The point in which the lines of movement intersect within the field of view of the telescope would, therefore, be the position of the image reflected from the true octahedron face. Measured in this way the octahedron angle of alum is found to be the theoretical angle 70° 31¾′.

The images do not move continuously, but *per saltum*, indicating that the reflecting planes are vicinal faces which probably possess rational indices, and must, therefore, be inclined at certain definite angles to the octahedron face; but the indices are very high numbers.

Observations upon sodium chlorate, zinc sulphate, magnesium sulphate, and other substances, show that other crystals exhibit the same behaviour. The faces of a crystal are in general not faces with simple indices, but vicinal planes slightly inclined to them, and they change their inclinations during the growth of the crystal; they also change their inclinations when the crystal is immersed to a greater or less depth in the solution.

Every point within a crystal has at some time been a point on the surface, and has been subject to the conditions of equilibrium between crystal and solution which prevail there. It is believed by the author that a study of the vicinal planes and of the liquid in contact with them, may lead to some understanding of these conditions.

In order to ascertain the composition of the liquid, attempts were made to determine its refractive index by means of total reflection within the crystal. This appears, indeed, to be the only method which can give direct information concerning the ultimate layer in contact with the growing face, and it is somewhat remarkable that it has not been applied before. Considerable difficulty was experienced in making this measurement, but ultimately good readings were obtained, which gave the value 1·34428 as the refractive index in sodium light, at 19° C., of the liquid in contact with a growing crystal of alum. The refractive indices of a series of solutions of known strength, ranging from dilute to supersaturated, having been previously measured, the above index was found to correspond to a liquid containing about 10·80 grammes of alum in 100 grammes of solution. A saturated solution at 19° C. was found to have the refractive index 1·34250, and to contain about 9·01 grammes of alum in 100 grammes of solution.

Sodium chlorate was examined in the same way; it was found that the liquid in contact with a growing crystal has at 19° C. the index 1·38734, and contains about 47·8 grammes of salt in 100 grammes of [redacted]on; a saturated solution of sodium chlorate at 19° C. has the 1·38649, and contains about 47·2 grammes of salt in 100 grammes ion.

The liquid in contact with a growing crystal of sodium nitrate has at 19° C. the index 1·38991, and contains about 48·45 grammes of salt in 100 grammes of solution; a saturated solution at 19° C. has the index 1·38905, and contains about 48·1 grammes of salt in 100 grammes of solution.

In each case the liquid in contact with the growing crystal is slightly supersaturated. It was not found to exhibit double refraction even in the case of sodium nitrate. No experiments seem to have previously been made upon the nature of this liquid.

G. Wulff has suggested that vicinal faces are due to concentration streams in the solution. In order to test this view, crystals of alum were measured after growing for several hours in solution kept continually agitated in order to eliminate the action of the concentration streams. Almost no effect was produced upon the angles of the vicinal faces.

In sodium chlorate and sodium nitrate the solute is about 45 times more dense in the crystal than in the adjacent liquid. Now planes with high indices in a space-lattice contain fewer points in unit area than planes with simple indices. The author suggests that vicinal faces grow upon a crystal in preference to simple forms because the crystallising material descends upon the growing face in a shower which is not very dense.

"On the Dependence of the Refractive Index of Gases on Temperature." By George W. Walker, M.A., Fellow of Trinity College, Cambridge. Communicated by Professor J. J. Thomson, F.R.S. Received February 26,—Read March 26, 1903.

(Abstract.)

The investigations of Professor Mascart on this subject are perhaps the most extensive of any up to the present time. He examined the effect in several gases, and found that in general the temperature coefficient exceeded the theoretical coefficient given by Gladstone and Dale's law. The range of temperature was, however, comparatively small, and his results for air do not agree with those of Lorenz, von Lange, and Benoît. In fact these four observers disagree. Lorenz and Benoît found a coefficient agreeing with the above law, while von Lange obtained a coefficient less than the theoretical value.

A repetition of the measurements therefore seemed desirable. The gases examined were air, hydrogen, carbon dioxide, ammonia, and sulphur dioxide. The range of temperature was from 10° C. to

100° C. The method used was the well-known one of Jamin, but special precautions were taken to obtain accuracy, and to be sure that the gas had not changed in composition during the various changes of pressure and temperature to which the containing tubes were subjected. An accuracy of about one part in 600 has been obtained.

The results are briefly shown in the following table, and it will be observed that the temperature coefficients obtained are substantially less than those obtained by Mascart.

Absolute Value of μ for the D line at 760 mm, and 0° C.

Observer.	Air.	Hydrogen.	Carbon dioxide.	Ammonia.	Sulphur dioxide.
Mascart......	1·0002927	1·000139	1·000454	1·000379	1·0007038
Lorenz.......	—	1·000139	—	1·000373	—
Ketteler	—	1·000143	1·000449	—	1·000686
Dulong	1·000294	1·000138	1·000449	1·000385	1·000665
Walker	1·0002928	1·0001407	1·0004510	1·0003793	1·0006758
	±3	±15	±5	±5	±4

“On the Evolution of the Proboscidea.” By C. W. ANDREWS, D.Sc. Communicated by Professor E. RAY LANKESTER, F.R.S. Received March 5,—Read March 26, 1903.

(Abstract.)

Until the author’s recent discoveries of primitive Proboscidea in the Middle and Upper Eocene formations of the Fayum, Egypt, the oldest known members of this mammalian order were *Dinotherium cuvieri* and *Tetrabelodon angustidens*, from the base of the Miocene in France. The new Egyptian fossils not only reveal for the first time the early history of the order, but also provide more satisfactory material for the discussion of its evolution than has hitherto been available.

The most important changes in the Proboscidea occur in the skull, mandible, and dentition.

Owing to the increase in the size of the tusks and to the presence of the proboscis, the facial region of the skull becomes shortened, and at the same time the premaxillæ become wider. The presence of the proboscis also accounts for the position of the external nares. The demand for a greater surface of attachment for the muscles supporting a skull rendered heavy by the tusks and trunk, is met by the great development of the diploë in certain of the cranial bones, resulting in the enormous expansion of the forwardly sloping occipital surface. The maxillæ become greatly enlarged concomitantly with the increase in the size and degree of hypselodonty of the molars. At the same time the zygomatic arch becomes weaker and the jugal takes a smaller share in its composition.

The mandible is at first short and stout, with a massive symphysis. Afterwards it becomes more and more elongated as the stature of the animals increases; and this elongation is for the most part effected by the lengthening of the symphysial region, though the backward rotation of the ascending ramus tends to the same end. The prolongation of the mandible beyond the premaxillæ must have been covered by a proboscis-like structure composed of the upper lip and nose, probably more or less prehensile at its extremity. The lengthening of the mandible seems to have reached its maximum degree in the Middle Miocene, after which it again became shortened by the reduction of the symphysis, while the fleshy and now mobile proboscis was left behind as the sole organ of prehension.

In the upper dentition the chief changes are the loss of incisors Nos. 1 and 3, and the great increase in size of incisor No. 2, which eventually forms the great tusk characteristic of the later Proboscidea. The canines are soon lost. In the earliest forms, some at least of the cheek-teeth (milk-molars) are replaced by premolars in the usual manner, and these teeth remain in wear simultaneously with the true molars; but in later forms no vertical succession takes place, and as the milk-molars are worn they are shed, being replaced from behind by the forward movement of the molars. Of these also the anterior may be shed, until at length in old individuals of the later types the last molar is alone functional. The gradual increase in the complexity of the proboscidean molars is one of their most striking characteristics. All stages can be traced between the simple, brachyodont, bilophodont (quadritubercular) molars of *Mœritherium* (Middle Eocene) to the extraordinarily complex type of tooth found in *Elephas*. Thus in *Palæomastodon* (Upper Eocene) the molars are trilophodont, and the same is true of the first and second molars of *Tetrabelodon* (Miocene), in which, however, the last molar is complicated by the addition of further transverse crests. In the Stegodonts of the Siwalik Hills (Pliocene) a further increase in the number and height of the crests takes place, and the whole crown of the tooth is more or less covered with a thick coat of cement. Still later, the transverse crests become highly compressed laminæ united by cement, and these are as many as twenty-seven in number in the Pleistocene *Elephas primigenius* and the recent *E. indicus*.

The evolution of the lower molars corresponds with that of the upper molars. Of the lower incisors the middle and outer pairs (Nos.

The authors considered it probable that much information might be obtained by the careful study of the cell groups in the spinal cord of a mammal differing markedly in its musculature from man, and as no previous observations on similar lines had been made in the same fulness on the spinal cord of any of the Cetacea, they describe the results of an examination of the cell groups in each segment of the cord of a member of this class, *Phocæna communis.* The investigation was carried out by obtaining a very recently captured specimen and at once preserving its tissues by injecting into its blood-vessels a solution of formalin, a method which has the advantage of preserving the natural configuration of the enlargements of the cord. A number of sections were prepared for the microscope by different methods from each segment of the whole cord, and typical sections were selected and photographed.

The principal features in which the musculature of the porpoise differs from that of man are the almost entire absence of a hind limb, the reduced musculature of the upper limb, and the possession of a large and flexible tail acted upon by powerful muscles, with some other differences noted in the text.

The segments of the cord giving origin to the nerves supplying these parts were compared with corresponding segments of the human cord. The groups of motor cells were found to be clearly differentiated from one another, and striking changes were found to occur in the shape of the grey matter and in the cell-groups as we passed from one segment of the cord to another, pointing clearly to a connection between the character of the part supplied with motor nerves from any segment, *e.g.*, limb, trunk muscles, genital muscles,—and the arrangement of the anterior horn cells in that segment.

The area of the grey matter and of the different columns of white matter was also determined at each segment.

The authors also describe some hitherto unrecorded features in the minute structure of the cord of this animal, especially the position of the nucleus of the spinal accessory nerve, and a detached mass of grey matter, probably corresponding to the vesicular column of Clark, in the lumbo-sacral region of the cord.

"Solar Prominence and Spot Circulation, 1872—1901." By Sir NORMAN LOCKYER, K.C.B., F.R.S., and WILLIAM J. S. LOCKYER, Chief Assistant, Solar Physics Observatory, M.A. (Camb.), Ph.D. (Gött.), F.R.A.S. Received March 17,—Read March 26, 1903.

[PLATES 6 AND 7.]

In our former communications* referring to the connection between solar, meteorological, and magnetic changes, some of the results obtained by the reduction of the solar prominences, as observed by Professor Tacchini at Rome, were described. It was stated that the curve representing the variation of percentage frequency of the prominences for the whole limb of the sun indicated that in addition to the main epochs of maxima and minima coinciding in time generally with those of the maxima and minima of the total spotted area, there were also prominent subsidiary maxima and minima.

Further, dividing the sun's limb into zones of 20° in width from the equator, with a polar zone of 10°, and dicussing each zone separately, the variation of the prominence percentage frequency about the equator was found to be very different from that in the higher latitudes, the former changing with the spots, and the latter exhibiting corroborated Carrington's results and extended the discussion of the observations up to the end of the year 1879.

The result of these two investigations showed that at sunspot maximum there was only one zone in each hemisphere in which spots were situated, the centre of this being about 18° N and S, while at minimum there were two zones existing simultaneously in each hemisphere; the older cycle dying out in the zone, the centre of which was situated in low latitudes, and the new one commencing in high latitudes, its centre being about latitude ± 30° to ± 35°.

Later observations extending up the present year have further corroborated these general deductions, for each hemisphere, and we are now quite familiar with this cycle of sunspot latitude variation.

In the present investigation, the fact has been brought out that the prominences also undergo an apparently regular variation of latitude throughout a period of about eleven years concurrently with the spots.

For the purpose of our inquiry, the object of which has been stated above, we have discussed independently of each other, two fine series of prominence observations, one made by Tacchini at Rome extending from 1872 to 1900, and the other by Ricco and Mascari at Catania from 1881 to 1901.

Both these series were handled in the same way, and both indicated similar changes of latitude of prominence action, showing that the variations recorded were real and not due to any personality of the observer or difference in the method of observation.

The data for the discussion of the solar prominences as observed by Tacchini have been taken from the same source as before,* while those of Ricco and Mascari are published in and have been extracted from the same volumes.

We may here take the opportunity to express our thanks to Professor Ricco, with whom we have been in communication, and who has very kindly forwarded for our use some unpublished data concerning his prominence observations and reductions.

The method of reduction adopted was to determine for each year the percentage frequency of prominence activity for every 10 degrees of solar latitude north and south. A series of curves was next drawn, one for each year, the abscissæ representing the latitudes of prominences north and south, and the ordinates their percentage frequency. It was then found that the centres of prominence activity, or, in other words, the maxima of the curves, were sometimes single, sometimes double, and in one or two cases even triple in each hemisphere. This suggested that just as sometimes there are two

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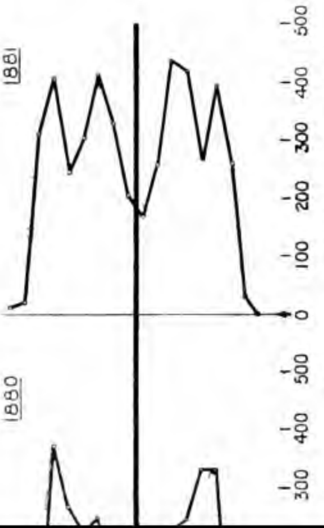
1874—Dec., 1879.' 'Publicationen des Astrophysikalischen Observatoriums zu Potsdam,' Band I and II.

* 'Società Spettroscopisti Italiani,' vol. 1, 1872, to vol. 20, 1900.

zones of spots existing at one time, so there might be one, two, or occasionally three zones of prominences in existence in each hemisphere simultaneously.



ge frequency of prominence activity for every 10° of
, from Tacchini's observations.

Further, a close examination of the whole set of curves with reference to these points of maxima made it possible not only to study the changes of latitude of these points from year to year and their positions when commencing to develop or about to disappear, but the intensity of these centres in relation to each other.

The accompanying illustration (fig. 1) shows the curves drawn for the years 1879, 1880, and 1881, from the observations of Tacchini, and serves as an example of the curves that have been discussed; they exhibit the change from a single to a double subsidiary maxima for the whole period covered by the observations, the results illustrated graphically in Plates 6 and 7 were obtained.

In these figures the facts are brought together for each hemisphere separately. The medials of the lines (curves A and B) show the heliographic latitudes of the centres of prominence action; the thickness of these lines represents the relative percentage frequency of prominence action.

For the sake of comparison, three other curves for each hemisphere are given. The first (curve C) shows the mean heliographic latitude of spotted area for each hemisphere. For the construction of these, the values, since 1873, have been extracted from the Greenwich Reductions,* but previous to that date the values have been obtained from Mr. Marth's reductions,† and those completed at the Solar Physics Observatory from measures supplied by Professor Backlund, of the Wilna Observatory.

The next curve (curve D) illustrates the variations of the percentage frequency of prominence action for each hemisphere taken, as a whole, and is similar to those given in our former papers.

The last curve (curve E) shows the variation of the mean daily area of sunspots from year to year, also for each hemisphere.

Referring now to the changes of latitude of the prominence centres of activity, both series of curves for the north as well as for the south hemisphere exhibit the same general features.

The first conclusion illustrated by the curves is that prominence activity in the main has a poleward drift, that is, the change of position of the zones of activity is in the direction from low to high latitudes. In some years, the centres of activity appear to form two zones in each hemisphere at about latitudes ± 24° and ± 50°, which eventually amalgamate at about latitude ± 40° and move polewards, fading out in about ± 70° to ± 80°. As this zone disappears in high latitudes a new zone at about latitude ± 20° begins, and this after a few years becomes associated with another zone in about latitude ± 50°, and eventually amalgamates with it.

The epochs at which these different zones come into play in relation to the general curve of prominence activity for the whole hemisphere are as follows: From a little after the maximum of prominence activity to just before the minimum, two zones in the latitudes ± 24° and ± 50° are in existence and of decreasing intensity. Before the minimum is reached these two zones amalgamate in about latitude ± 40°. At the minimum there is only one zone, and this of small intensity. Between the minimum and the following maximum this zone rapidly takes a northern movement, increasing in intensity; a new outburst occurs in a zone nearer the equator (latitude ± 24°), which also increases rapidly in intensity.

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* 'Spectroscopic and Photographic Observations made at Royal Observatory, Greenwich, 1884,' and after.

† MSS. at Royal Society.

After these general statements, we now refer to some details showing that there are some variations from the above generalisation.

For these details the curves deduced from both sets of observations made by the different observers are so very similar that it does not matter which are examined.

Attention may first be drawn to certain differences between the curves representing the latitude variation for the two hemispheres. It will be noticed that for the period 1872—1882, the curves for both hemispheres are very similar. We next consider the period 1880—1893. Here there are differences between the two hemispheres. The curve for the northern hemisphere resembles very closely that for the preceding period, but it differs somewhat from its corresponding curve for the southern hemisphere. The corresponding northern zone in latitude 45° is missing from the southern hemisphere, while a zone of activity nearer the equator about latitude 24° is present. Further, the polar zone for the southern hemisphere continues to be prominent for two years longer than that in the opposite hemisphere.

In the succeeding curves, which extend from 1891—1901, both by Father A. Fényi, S.J., who has published* the individual observations, and the reductions of the positions and frequency of prominences observed at Haynald Observatory for the years 1884 to 1890 inclusive. He gives curves constructed somewhat after the manner adopted in the present enquiry, as illustrated above, in fig. 1. A comparison of the points of maxima from his curves with those of Tacchini and Ricco and Mascari for the period common to all three sets of observations is made in the following tables, each hemisphere being given separately. The vertical columns show, for each year, the heliographic latitudes of the points of maxima, and an asterisk (*) is placed against the one which is the more or most prominent in each hemisphere; when there are two, and they are of equal intensity, this symbol is attached to each, while in the case of only slight indications of maxima the latitude is enclosed in brackets.

Northern Hemisphere.

	1884.	1885.	1886.	1887.	1888.	1889.	1890.
Tacchini....	50, *25	45, *25	45, *20	35	*35, 15	40	45
Ricco and Mascari	55, *20	55, *15	45, *25	30	35	45	45
Fényi	65, 45, *15	45, *25	45, *20	45	*40, 20	*43, 25	45

Southern Hemisphere.

	1884.	1885.	1886.	1887.	1888.	1889.	1890.
Tacchini..	(75), *25, 5	25	35, *20	*45, 25	*45, 25	45	45
Ricco and Mascari	(85), *25, (5)	25	35	*45, 25	*50, *25	45	50
Fényi	(75), *35, *15	(50), 35 *10	*35, *10	*50, 30, 15	*50, 25	40	*50, 20

It will be seen that for these seven years, Fényi's results are in very close accordance with those deduced from the other two series of observations, thus generally endorsing those portions of the curves in Plates 6 and 7 covering this period.

It was mentioned in a previous paper† that the mean prominence curve for each hemisphere exhibited subsidiary maxima and minima. In the light of the present investigation, it is interesting to compare this curve with that representing the changes of latitude of the zones of prominences. In every case, and for each hemisphere, the subsidiary maxima are coincident in time with the presence of two zones of prominences, each well-developed, while at the principal minima only one zone is in evidence.

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* 'Publicationen des Haynald-Observatoriums, Kalocsa,' Heft VI, 1892, und VIII, 1902.

† 'Roy. Soc. Proc.,' vol. 71, p. 244.

We have already explained the fact that spots are restricted to a zone having its limits at latitudes ± 5° and ± 35°, while prominences occur all over the sun's disc, even up to the poles, and also that spots always commence their cycle in high latitudes (about ± 35°) and gradually approach the equator until within 5°, when a new cycle is commenced in high latitudes. Prominences on the other hand begin in comparatively low latitudes (about ± 24°), and finish their cycle near the poles.

A glance at the Plates 6 and 7 brings out the interesting fact that at sunspot minima, when two zones of spots are in evidence, there is only one zone of prominences, while when only one zone of spots exists the prominences are for the most part confined to two zones.

The conclusions arrived at in the present communication may be summarised as follows:—

1. The centres of action of prominence activity undergo an apparently regular variation.

2. The direction of motion of these centres is from low to high

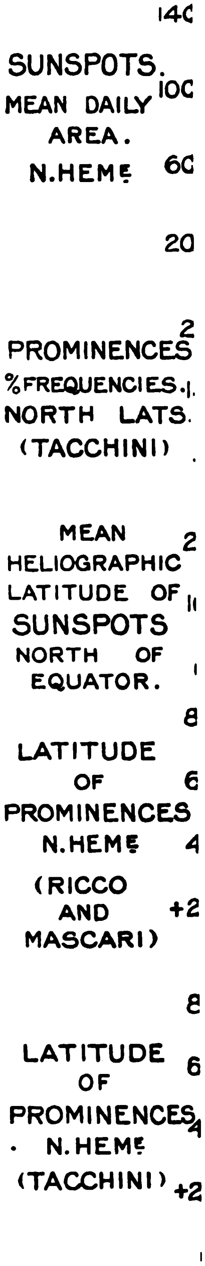


Plate 6.—Curves

Note.—The conti

Furthermore, it is to be observed that whenever this state of things seen, there is always, so far as we have observed, at least one con-iguous cell which is destitute of a nucleus (see figs. 1, 2, 4). We onvinced ourselves of this highly important fact by examining entire rothallia that had been carefully stained, as there was always the

FIG. 1.

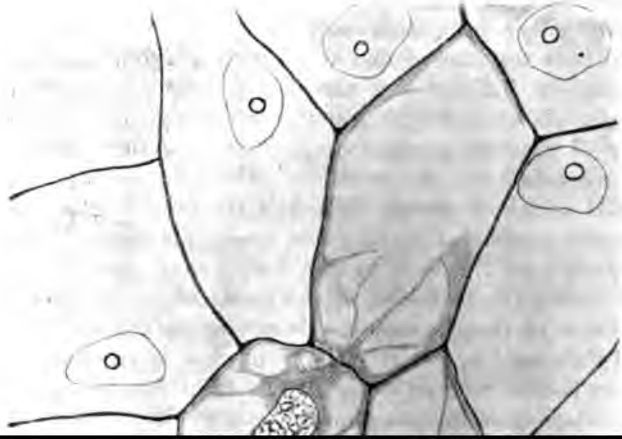


FIG. 2.

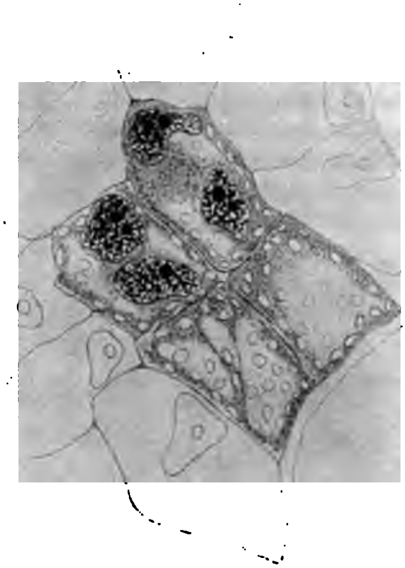
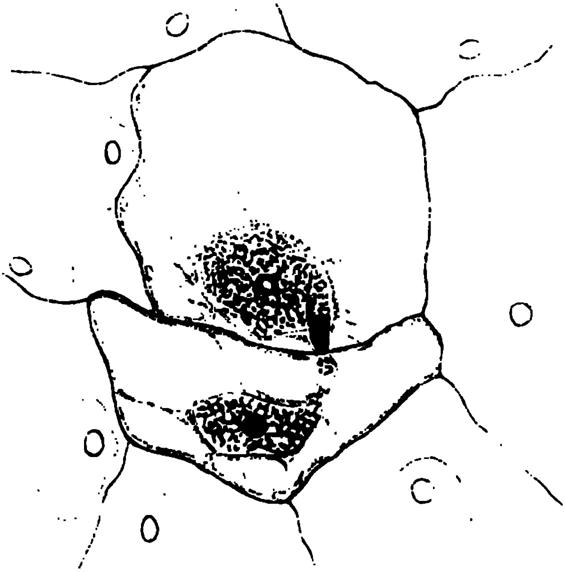


FIG. 3.

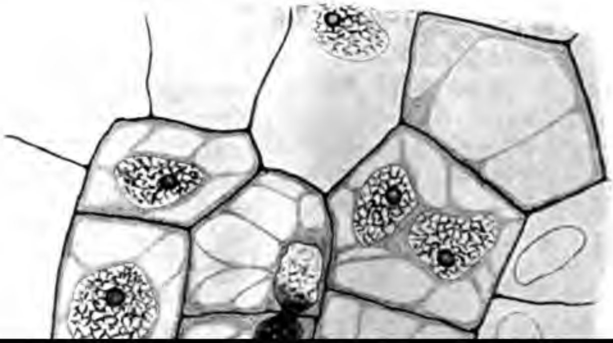


on through which a strand of cytoplasm was still visible connecting ne two cells.

When the migrating nucleus has passed into the neighbouring cell sometimes fuses at once with the nucleus already present there, ut often the two nuclei remain more or less separated for an ppreciable interval of time.

It appears then to be clear that the presence of the pair of nuclei not to be regarded in these cases as resulting from a division of the

FIG. 4.



irregular growth of the new tissue, and with the sporadic appearance of sporophytic members, on the prothallium already described by Lang.

When the nuclei of the cells in the apogamous regions are examined in course of karyokinesis, they are seen to possess a much larger number of chromosomes than those of the ordinary tissue cells of the prothallium. Owing, however, to the manner in which the very numerous chromosomes are distributed on the spindle, an exact estimation of actual numbers is a task of considerable difficulty. There appear, however, to be forty and eighty in the respective classes of nuclei.

We regard the whole process as a kind of irregular fertilisation. The doubling of the chromosomes receives an explanation strictly analogous to that afforded by the normal fusion of oosphere and spermatozoid. But instead of one cell only (the oospore) serving as the starting point for the new generation, a number of such units loosely co-operate to produce it. And in this connection it is perhaps significant that the young plantlet is commonly borne on, and produced from, a special sporophytic outgrowth, of which the constituent cells may have become homologously differentiated into a sort of pro-embryo.

Instances are not wanting amongst lower animals to show that close cellular in-breeding may occur, and form part of the normal sexual series of events. It is not desired to press the analogy of such cases; but the case of Actinosphærium shows that a process indistinguishable from normal sexual fusion may occur between sister cells that have only lately arisen from the division of a parent cell.

We do not propose to enter on a full theoretical discussion of the bearings of these observations at the present time, but we hope to do so when we are in a position to deal completely with the corresponding cytological features associated with apospory.

A Study of a Unicellular Green Alga, occurring in Polluted Water, with especial Reference to its Nitrogenous Metabolism." By HARRIETTE CHICK. Communicated by Professor RUBERT BOYCE, F.R.S. Received February 28,—Read March 12, 1903.

(From the Thompson Yates Laboratories, University College, Liverpool.)

[PLATE 8].

A small, unicellular, green alga was noticed to be frequently present n sewage, and sewage materials, when these had been kept for some ime. The same alga was also found to have seeded itself in dilute mmoniacal solutions. This somewhat peculiar habitat seemed to romise that something of interest might repay a study of the hysiology of the plant. On examination, under the microscope, it vas found to be an extremely small, unicellular, non-motile alga, with well-defined chromatophore, which latter contained a distinct yrenoid. The general characteristics of the alga pointed to its being losely allied to *Chlorella* (Beyerinck),* and I have provisionally placed in that genus. *Chlorella vulgaris* was originally described by Beyerinck as possessing either no pyrenoid, or an insignificant one; Chodat,† on the other hand, has described the species as having

The algal material also grew exceedingly well when stroked upon the following ammoniacal medium:—2·0 grammes NH_4Cl, 1·0 gramme Na_2CO_3, 0·5 gramme K_2HPO_4, 15·0 grammes agar-agar, in 1 litre water. The growth was, however, contaminated with numerous bacteria.

This evident ready assimilation of ammonia pointed to the possibility of the organism's playing some part in the "purification" of sewage or further natural "purification" of sewage effluents, and its nitrogenous metabolism was studied with this view.

Mode of Preparation of a Pure Culture.

The first step necessary towards a study of the chemical physiology of the alga was the preparation of a pure culture. As the separation of algæ in pure culture is not of very common occurrence, few as yet having been obtained, it may be permissible to describe in detail the method adopted. The following methods were tried without success. Sterilisation, as regards bacteria, was attempted by exposure of liquid cultures to sunlight, and hydrogen peroxide was also tried as a means of killing the adherent bacteria, but the alga appeared to be susceptible also. More purely bacteriological methods were employed, viz.:—by stroking in great dilution over the surface of potatoes, and by "pouring" plates of ammonium agar, or ammonium gelatine, and incubating in sunlight, at a low temperature. All these methods failed for one reason or another, but the organism was finally separated in the following way. It seemed advisable to employ an ammonium-containing medium, and ammonium agar was therefore selected. Plates of this medium were poured and allowed to set. An ammoniacal solution was also made and sterilised, having the following composition:—0·5 gramme Na_2CO_3, 0·5 gramme K_2HPO_4, 0·1 gramme NH_4Cl, 1 litre tap-water or distilled water. This solution will in future be alluded to as "solution A."

A few drops were allowed to drop upon the surface of the poured plates, then a small portion of the impure material was added to the first plate, and the whole was brushed over the surface. The same brush was then used to brush the liquid over the second plate, and so on, to six plates or more, without the addition of fresh algal material, so that a considerable dilution was obtained. The plates were kept in the light, in as much sunshine as was possible, protected from dust, and in a damp atmosphere. In seven to fourteen days, green growth was generally visible, and after three or four weeks, definite algæ-colonies were to be distinguished among the colonies of bacteria.

In this way pure cultures of *Chlorella pyrenoidosa* were obtained from

(1) Material of Ducat Filter Bed (Hendon).

(2) Sludge wash-water (Leeds).

This method may be relied upon to yield pure cultures, for it has since been repeated, and again proved successful.

The following is a fairly comprehensive diagnosis of *Chlorella pyrenoidosa*.

Chlorella (Beyerinck)* very minute, unicellular, non-motile, spherical or elliptical green algæ; chromatophore single, parietal, with or without a pyrenoid; cells isolated, showing no tendency to form colonies; multiplication by division of the cell-contents to form a number of daughter cells.

C. pyrenoidosa (sp. n.) :—cells spherical, 3—5 μ in diameter, sometimes attaining 11 μ; chromatophore single, parietal mantle-shaped, covering nearly the whole of the cell-wall; pyrenoid conspicuous and single (see Plate 8, fig 3); reproduction by successive division of the cell-contents to form within the mother cell as many as eight daughter cells, which subsequently become free of the mother cell wall (see fig. 5).

Physiological characters.—Showing a marked preference for ammonia and ammoniacal compounds (*e.g.*, urea) compared with nitrates in its culture fluids; growth and multiplication largely increased by the addition of glucose to the culture fluids, causing the cells and cell-contents to assume a changed and characteristic appearance.

Chlorella pyrenoidosa resembles *C. protothecoides* (Kruger†) very stimulated, and this is particularly rapid when grown upon nutrient agar or gelatine to which glucose has been added. The liquid medium *par excellence* for studying this organism, is sterilised sewage, in which it preserves its normal form and characteristics. In solution A, it also grows well, while if to the liquid a small quantity of glucose is added, the growth is much stimulated, and the individuals assume the appearance, described below, characteristic of growth in glucose media.

Accompanying the much more abundant development, the nitrogen assimilation is also increased, as will be seen later.

Glucose-containing media (fig. 4). When grown upon media containing glucose, while the general growth is much improved, and the individual cells are also larger in size upon the whole, the green colour of the cell-contents is much paler, and sometimes almost disappears, while the chromatophore is much disorganised. The contents of the cell appear to be segregated into a variable number of slightly refractile bodies, usually pale greenish-yellow in colour, which are apparently free in the cell. If when mounted in water under the microscope, a slight pressure is applied to the coverslip, the cell envelope is easily ruptured, and these bodies are liberated, and float freely in the surrounding liquid. The protoplasm also, in the case of glucose cultures, shows small granules, and the pyrenoid is no longer to be traced, but a certain amount of staining with iodine may be noticed in the granular protoplasm. These changes closely resemble those noticed respectively by Krüger[*] in the case of his *Chlorothecium saccharophilum* and *Chlorella protothecoides*, and by Matruchot and Molliard[†] in the case of *Stichococcus bacillaris*, when these algæ were grown upon glucose containing media.

The results of growing *C. pyrenoidosa* upon media containing lactose or saccharose also resembled those of the above authors. These two sugars appear to possess a far lower nutritive value than does glucose, while the algal cells preserve their normal appearance. When grown upon ammonium agar, the growth is comparatively slow, the individuals appear larger in size than when grown upon ordinary nutrient agar, their green is darker and the cell-contents are of the normal type.

Quantitative Chemical Experiments.

The evident preference of this organism for ammoniacal culture solutions seemed to be a reason for studying its nitrogen assimilation. The absorption of ammonia by algæ is not a new discovery; it is well known, from the researches of Letts and Hawthorne,[‡] that *Ulva latissima* absorbs ammonia with extreme rapidity, and, consequently, will grow and develop to an extraordinary extent in sewage-polluted water, for such water contains a comparatively large amount of ammonia. *C. pyrenoidosa* also absorbs ammonia in a marked degree, and this fact lends practical importance to the study of its nitrogenous metabolism, for it also may have some bearing upon the purification of sewage. Although the organism is itself very small, and the absolute quantities it tackles also insignificant, yet the results of the following experiments, themselves upon a very small scale, tend to show that if *C. pyrenoidosa* were growing in great quantities (*e.g.*, in the bed of a river polluted with sewage), very important changes might be effected in the composition of the water in which it grew.

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[*] Zopf's 'Beiträge z. Morph. u. Phys. Nied. Org.,' Leipzig, vol. 4, 1894, p. 91.

[†] 'Rev. Gén. de Bot.,' vol. 13, 1902.

[‡] Letts and Hawthorne, 'Roy. Soc. Proc., Edin.,' 1901, p. 268; Letts, "Report on the Scheme of Sewage Purification proposed for Belfast, and its Probable Effects on the Lough."

The method of pure cultures was adopted in every case, unless otherwise stated. "Pasteur flasks" were used to contain the cultures; these were sterilised and then filled with the sterilised culture fluid by means of sterilised pipettes. One flask was always kept sterile, as a control, while a second was inoculated with *Chlorella pyrenoidosa*. The flasks were kept, often for months, in a sunny place, protected from dust, and in a damp atmosphere in order to prevent evaporation. These Pasteur flasks can be obtained to hold 300 c.c., and this size, when containing 150 c.c. of fluid, was found to be extremely convenient. To see if the cultures and operation, their ends were fixed by means of cotton wool in the mouths of test tubes, and the whole wrapped in filter paper, and left so until actually used. When the samples were removed for analysis, the liquid was at the same time tested for sterility in the way already described.

The culture liquids employed were sterilised sewage and sewage effluents, and also sterilised artificial liquids whose composition was arranged to resemble that of sewage as nearly as possible. The sewage was usually allowed to stand for several days, then filtered, and afterwards sterilised by successive heatings at 100° C. The artificial culture media were the solution, A, referred to above, and also the same solution to which a small amount of glucose has been added (about 0·25 per cent.).

The methods of analysis were those usually adopted in water and sewage analysis. In the estimation of the free ammonia a small portion of the liquid to be analysed (3 to 10 c.c.) was diluted with about 500 c.c. ammonia free water and distilled. Three successive portions of 50 c.c. were distilled off, and the ammonia contained in them estimated by means of adding Nessler's solution and comparing the yellow tint with that given by definite amounts of standard ammonium chloride solution. "Albuminoid ammonia" was afterwards estimated by adding a fixed amount of "alkaline permanganate" solution,[*] distilling as long as ammonia came over in the distillate, and estimating these amounts in the same way. It is possible that in some cases the quantities of albuminoid ammonia given in the analyses in the following tables may be slightly too low. For, in certain instances, towards the end of the distillation, the ammonia came over, in the distillate, very slowly and in very small quantities. It was considered more accurate in these cases to discontinue the analysis when these amounts fell below a certain very small value, since the error in estimating such very small quantities becomes comparatively great.

The presence of nitrates or nitrites was detected by means of diphenylamine-sulphuric acid and metaphenylene-diamine, while quantitative estimations were made by means of the "copper-zinc couple" method in which the nitrogen present as nitrates or nitrites is estimated as ammonia.[†] To allow for any ammonia that might have been originally present in the solution or introduced as traces during the analysis, a control estimation was always made. This analysis was carried out in every way like the real one, except that no "couple" was introduced, and the ammonia thus obtained was subtracted from that found in the actual estimation.

The analysis of the cultures of this alga by these methods is by no means easy, for the quantities treated are exceedingly small, the methods of analysis adopted demand a comparatively large margin of experimental error, and the length of time, over which many of the experiments must continue, is also a drawback. The sterilisation of flasks and pipettes must also introduce a slight error where such small quantities of ammonia are concerned. The estimation of the albuminoid ammonia is frequently rendered troublesome by the tendency of the liquid to bump while distilling. It has been found that this difficulty is usually obviated if the ammonia-free water used for dilution is well aërated, and if, from time to time, small quantities of fresh aërated, ammonia-free water are added during the distillation.

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[*] 'Volumetric Analysis,' Sutton, 8th ed., p. 512.

[†] *Ibid.*, p. 482.

The absorption of ammonia, as this alga grows in ammonia-containing culture liquids, is well shown by the results of the analyses in Tables I—VII. At the same time, the amount of albuminoid ammonia present steadily increases, and, on the whole, a fairly even balance is maintained, which can be seen by comparing the quantities of total nitrogen.

The evident preference of *Chlorella pyrenoidosa* for its nitrogen as ammonia, rather than in an oxidised form, is seen from Tables II, III, IV. In Table III an impure culture of the alga was allowed to grow alongside the pure culture, and it will be seen that similar changes allowed to go free in the liquid. With the object of settling this point, cultures which had manufactured a good deal of albuminoid ammonia, were subjected to centrifugalisation, and analyses made of the clear solution (see Tables VI *b* (analysis No. 3) and V). The clear solution in either case contained no more albuminoid ammonia than the control, proving that the elaborated nitrogenous substances were entirely retained within the cell body.

In two cases, however (Tables VI *b* (analysis No. 4) and VII *b*), older cultures were examined. The word "older" is here used to imply that these cultures had passed through many more cell-generations, judging from the appearance of the growth and the size of deposit present. In these cases, a considerable amount of albuminoid ammonia was found to exist in the clear liquids. This seemed to prove that the algal individuals, under certain conditions, yield up to their culture liquid somewhat complicated nitrogenous matters in a soluble form, and a probable explanation would be that they exhibit this phenomenon when the cells are in a dying or dead condition. This fact assumes a certain significance when it is remembered that the natural habitat of this alga is ammoniacal liquids, hence notably polluted waters. The actual amount of assimilation and excretion is extremely small, and it would be difficult to assert that the presence and growth of such an alga as *Chlorella pyrenoidosa* was of very great importance in nature. But the effect of this alga, if present, would probably be to leave the water, in which it has grown, in what would be termed a "more impure" condition. It is evident from the above experiments that the organism possesses the faculty of converting saline ammonia into albuminoid ammonia, which, under certain conditions, is discharged from the cells into the culture fluid. "Albuminoid ammonia" is a name given to a certain class of substances on account of facts connected with their analysis; these substances are quite unstudied, and, in this instance, may be perfectly harmless compounds. But the presence of albuminoid ammonia to more than a very small degree has always been considered, perhaps without sufficient foundation, to show evidence of dangerous pollution, and hence to be most prejudicial to any water. The formation and excretion of such compounds by a green alga appears to be a new phenomenon, and is not without especial interest in this connection.

An attempt was made to compare the nutritive value for *Chlorella pyrenoidosa* of various nitrogenous substances, and with this object a series of nutrient fluids were prepared of the same composition as those used by Krüger[*] in his experiments with *Chlorella protothecoides* and *Chlorothecium saccharophilum.*

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[*] Krüger, Zopf's 'Beiträge z. Phys. u. Morph. Nied. Org.,' Leipzig, vol. 4, p. 103.

A stock solution was made having the following composition:—

Glucose 1·0 per cent., K_2HPO_4 0·2 per cent., $MgSO_4$ 0·04 per cent.,
$CaCl_2$ 0·02 per cent., distilled water 100 c.c.

With this stock solution eight different culture solutions were prepared containing respectively 0·1 per cent. of the following:—

1. Asparagine.
2. Aspartic acid.
3. Urea.
4. Uric acid.
5. Peptone.
6. Xanthin.
7. Hippuric acid.
8. Stock solution, without further addition.

Of these solutions, the cultures grown in those containing urea and uric acid were found to flourish exceedingly well, and by far the best, while of the others, the culture in the liquid containing xanthin gave evidence also of being very well nourished.

Uric Acid Culture.—An analysis was made of this culture in the ordinary way. In the case of the control liquid, it was found that no free ammonia was obtained, and comparatively very little of the nitrogen of the uric acid was yielded as albuminoid ammonia. It was found, however, that when *Chlorella pyrenoidosa* had been growing in

Table I.—Showing Assimilation of Ammonia.

Culture in Solution "A." Started October 21, 1901.

No. of analysis.	Date of analysis.	—	Control.	Culture of *Chlorella pyrenoidosa.*
			Per cent. nitrogen.	Per cent. nitrogen.
1	21.10.1901	Free ammonia	0·00209*	—
2	26.10.1901	Free ammonia	0·00183*	0·00144*
3	2.11.1901	Free ammonia	0·00152*	0·00114*
4	28.11.1901	Free ammonia Albuminoid ammonia Nitrites Nitrates	0·00194† 0·00062 0 0	0·00071† 0·00081 0 0

* Estimated by direct Nesslerisation.

† Estimated, after distilling, by Nesslerisation.

Table II.—Showing the Assimilation of Ammoniacal Nitrogen in preference to Oxidised Nitrogen.

Culture in a Sterilised Sewage Effluent (Land).

Date of analysis.	—	Control.	Culture.
		Per cent. nitrogen.	Per cent. nitrogen.
31.10.1901	Free ammonia Albuminoid ammonia	0·000142 trace	— —
7.12.1901	Free ammonia Albuminoid ammonia Nitrates Nitrites Nitrogen—Total (by addition)	0·00009 0·00007 0·00215 0 0·00231	0 0·00025 0·00213 Faint trace 0·00238

Table III.—Showing the Assimilation of Ammoniacal Nitrogen in Preference to Oxidised Nitrogen, and Formation of Albuminoid Ammonia.

Culture in Sterilised Sewage, which had stood for some time. Started November 8, 1901.

Date of analysis.	—	Control.	Pure culture of *Chlorella pyrenoidosa.*	Culture of *Chlor. pyrenoidosa* contaminated with the bacteria which usually accompany it.
		Per cent. nitrogen.	Per cent. nitrogen.	Per cent. nitrogen.

Table IV.—Showing the Assimilation of Ammoniacal Nitrogen in Preference to Oxidised Nitrogen, and the Formation of Albuminoid Ammonia.

Culture in Sterilised Sewage. Started October 31, 1901.

No. of analysis.	Date of analysis.	—	Control.	Pure culture of *Chlorella pyrenoidosa.*
			Per cent. nitrogen.	Per cent. nitrogen.
1	31.10.1901	Free ammonia	0·00247	—
		Albuminoid ammonia	0·00041	—
2	20.10.1901 and 21.10.1901	Free ammonia	0·00231	0·00237
		Albuminoid ammonia	0·00072	0·00059
3	20.12.1902 21.12.1902 and 22.12.1902	Free ammonia	0·00276	0·00082
		Albuminoid ammonia	0·00063	0·00230
		Nitrites	Small reaction	Small reaction, but greater.
		Nitrites and nitrates	0·00128	0·00131
		Total nitrogen (by addition)	0·00467	0·00443

Table V.—Showing the Assimilation of Ammoniacal Nitrogen, Formation of Albuminoid Nitrogen, and the Results of Centrifugalisation Experiments.

Culture in Sterilised Sewage, containing no Oxidised Nitrogen. Started August 4, 1902.

No. of analysis.	Date of analysis.	—	Control.	Culture of *Chlorella pyrenoidosa*.	
				Whole culture.	Clear solution after centrifugalisation.
			Per cent. nitrogen.	Per cent. nitrogen.	Per cent. nitrogen.
1	4.8.1902	Free ammonia Albuminoid ammonia	0·00251 0·00033		

Tables VI (*a*) and (*b*).—Showing increased Nitrogen Assimilation when Glucose is present in the Culture Fluid, and the Result of Centrifugalisation Experiments, in the latter instance, both in the case of Younger and Older Cultures.

Table VI (*a*).—Culture in Solution "A." Started November 3, 1903.

No. of analysis.	Date of analysis.	—	Control.	Culture of *Chlorella pyrenoidosa.*
			Per cent. nitrogen.	Per cent. nitrogen.
1	3.11.1902	Free ammonia	0·00216	
		Albuminoid ammonia	0·00042	
		Nitrites and nitrates	0·00	—
		Total nitrogen (by addition)	0·00258	
2	21.11.1902 and 22.11.1902	Free ammonia	0·00231	0·00202
		Albuminoid ammonia	0·00040	0·00039
		Nitrites and nitrates	0·00	0·00
		Total nitrogen	0·00271	0·00241

Table VI (*b*).—Culture in Solution "A," to which a small amount of Glucose had been added (about 0·25 per cent.). Started November 3, 1902.

No. of analysis.	Date of analysis.	—	Control.	Culture of *Chlorella pyrenoidosa.*	
				Whole culture.	Clear solution after centrifugalisation
			Per cent. nitrogen.	Per cent. nitrogen.	Per cent. nitrogen.
1	3.11.1902	Free ammonia Albuminoid ammonia Nitrites and	0·00241 0·00045 0·00	— — —	— — —

Tables VII (*a*) and (*b*).—Showing increased Nitrogen Assimilation when Glucose is present in the Culture Liquid, and the Result of Centrifugalisation Experiments in the latter case.

Table VII (*a*).—Culture in Solution "A." Started August 5, 1902.

No. of analysis.	Date of analysis.	—	Control.	Culture of *Chlorella pyrenoidosa.*
			Per cent. nitrogen.	Per cent. nitrogen.
1	5.8.1902.	Free ammonia	0·00268	—
		Albuminoid ammonia	0·00012 (?)	
		Nitrites and nitrates	0·00	
		Total nitrogen (by addition)	0·00280	
2	19.11.1902.	Free ammonia	0·00221	0·00166
		Albuminoid ammonia	0·00039	0·00091
		Nitrites and nitrates	0·00	0·00
		Total nitrogen	0·00260	0·00257

Table VII (*b*).—Culture in Solution "A," to which a small amount of Glucose had been added (about 0·25 per cent.). Started August 5, 1902.

No. of analysis.	Date of analysis.	—	Control.	Culture of *Chlorella pyrenoidosa.*	
				Whole culture.	Clear liquid after centrifugalisation.
			Per cent. nitrogen.	Per cent. nitrogen.	Per cent. nitrogen.
1	5.8.1902	Free ammonia	0·00254	—	—
		Albuminoid ammonia	0·00015 (?)		
		Nitrites and nitrates	0·00		
		Total nitrogen	0·00269		
2	23.10.1902	Free ammonia	0·00231	0·00070	
		Albuminoid	0·00022	0·00141	

when offered in the form of ammonium salts, while the latter alone could assimilate nitrate nitrogen. Artari,[*] working with the gonidia of two lichens, isolated in pure culture, showed that, after peptone, asparagine and ammonium sulphate were the useful sources of nitrogen for these algæ, and other observers[†] have made equally careful experiments in the case of some of the higher plants, and have shown that not only simple ammonium salts, but also compound ammonias, such as methylamine, ethylamine, can readily be assimilated.

From a physiological point of view *Chlorella pyrenoidosa* must be included in this second class, for it has been shown, as the result of quantitative as well as qualitative experiments, that this alga prefers its nitrogen to be presented to it in the form of ammonia or ammoniacal compounds. Among the latter urea, uric acid, &c., rank high in nutritive value.

It would also appear from the foregoing chemical experiments that this ammonia, after being absorbed by the cell, is elaborated into albuminoid ammonia,[‡] a term used to describe certain nitrogenous bodies of ammoniacal nature, which yield ammonia when boiled with alkaline permanganate of potash; in fact, almost all the nitrogen assimilated would appear to remain in this comparatively simple form. This nitrogen, for example, is more easy of attack than that contained in uric acid. These elaborated nitrogenous compounds appear to be retained wholly within the cell body, but under certain conditions, only observed in the case of "older" cultures, they seem to escape from the cell, and can be traced free in the liquid.

The presence of glucose in a culture liquid frees the alga from the necessity it would otherwise experience of manufacturing carbohydrate for itself. The algal cell, being thus relieved of a certain part of its ordinary work, appears to be enabled to reproduce itself much faster, and its nitrogen assimilation is also much increased, though, owing to the increased multiplication of cells, it would be impossible to say that the nitrogen assimilation per cell was increased. The chlorophyll body of the cell, at the same time, gives evidence that its function has been interfered with by a most striking change in form and in the amount of chlorophyll. It is distinctly noteworthy that neither cane-sugar nor lactose can be substituted for glucose in this connection.

The effect of glucose in causing a definite change in the chlorophyll body, and in generally stimulating growth, would not appear to be an isolated fact. It has been shown by other observers in the case of three green algæ (*Chlorothecium saccharophilum*, *Chlorella protothecoides*, *Stichococcus bacillaris*), upon whose nutrition the effect of glucose was studied in pure culture, that there was a marked increase in growth when this carbohydrate was added to their culture medium, though no quantitative experiments were made to measure any difference in assimilation.

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[*] 'Bull. d. l. Soc. Imp. des Nat. de Moscou,' 1899, p. 39.

[†] Laurent, 'Ann. de l'Inst. Past.,' vol. 3, 1899; Lutz, 'Comptes Rendus,' vol. 126, p. 1227.

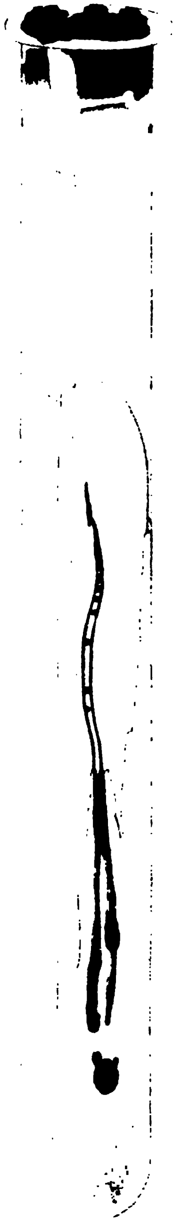
[‡] Wanklyn, 'Chem. Soc. Journ.,' 1867, p. 59.

The general features of the assimilation of nitrogen, on the other hand, and its subsequent history displayed by *Chlorella pyrenoidosa*, may be a specialised characteristic of this plant, having reference to its continual occurrence, in nature, in waters which contain a comparatively large amount of ammonia, notably in sewage and sewage-polluted waters. It is, however, impossible to express a definite opinion upon this point until the nitrogen assimilation of a series of many other plants has been studied in a similar manner.

DESCRIPTION OF PLATE 8.

Fig. 1.—Culture on nutrient agar (1 month).
,, 2.—Culture on glucose nutrient agar (1 month).
,, 3.—Cells from a culture on nutrient agar of 10 weeks.
,, 4.—Cells from a culture on glucose agar of 3 weeks.
,, 5.—Cells dividing, from a culture on nutrient agar of 10 weeks.

Chick. *Roy. Soc. Proc. Vol. 71, Pl. 8.*



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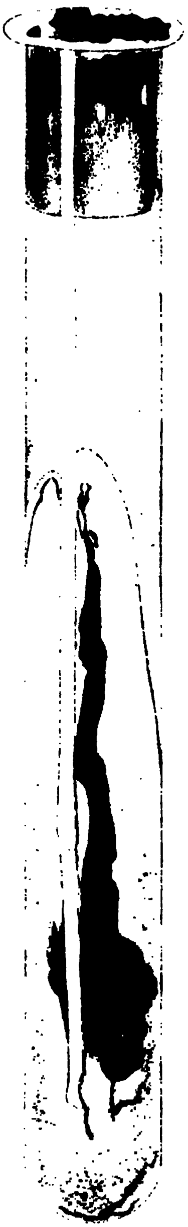

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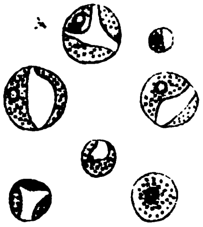

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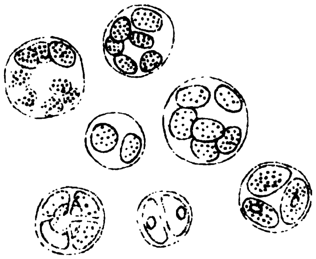

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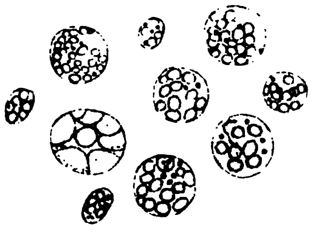

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"On *Lagenostoma Lomaxi*, the Seed of *Lyginodendron*." By F. W. OLIVER, D.Sc., F.L.S., and D. H. SCOTT, M.A., Ph.D., F.R.S. Received March 19,—Read May 7, 1903.

The existence in Palæozoic times of a group of plants (the Cycadofilices of Potonié) combining certain characters of Ferns and Gymnosperms, has been recognised for some years past by various palæobotanists.* The group in question embraces a number of genera, among which *Medullosa*, *Heterangium*, *Calamopitys* and *Lyginodendron* may be mentioned; the fern-like foliage of these plants is placed according to its external characters in the form-genera *Alethopteris*, *Neuropteris*, *Sphenopteris*, and others.

The evidence for the intermediate position of the Cycadofilices is extremely strong, but at present it is drawn entirely from a detailed comparison of their vegetative organs, especially as regards their anatomical characters. In no case, as yet, is the fructification of any member of the group known with certainty; such indications as have hitherto been detected are still in need of corroboration. Thus, the suggestion has been made that the large seed, *Trigonocarpon olivæforme*, may have belonged to some member of the genus *Medullosa*;† and in the case of *Lyginodendron* itself there is fairly strong reason to believe that one form of fructification (in the light of the observations to be described below, presumably the male) may have been of the *Calymmatotheca* type,‡ a type, however, of which the organisation is not yet fully understood. In the absence of satisfactory data as to the fructification, so high an authority as M. Zeiller has expressed a doubt whether the Cycadofilices were, after all, anything more than a specialised group of Ferns.§

A re-examination of the seeds, placed by Williamson in his genus *Lagenostoma*, has revealed unexpected points of agreement between the structure of the envelopes of certain of these seeds, on the one hand, and that of the vegetative organs of *Lyginodendron* on the other.

Two species of *Lagenostoma* (*L. ovoides* and *L. physoides*) were described by Williamson;* a third species, the subject of the present note, was left undescribed by him, though in his MS. catalogue he named it, after its discoverer, *Lagenostoma Lomaxi*, a name which we here provisionally adopt. This seed occurs in calcareous nodules of the lower Coal-measures, and chiefly at Dulesgate, in Lancashire.

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* Williamson, "Organisation of the Fossil Plants of the Coal-measures, Part XIII," 'Phil. Trans.,' B, vol. 178, p. 299; 1887; Solms-Laubach, 'Fossil Botany,' 1887, Engl. ed., pp. 141 and 163; Williamson and Scott, "Further Observations on the Organisation of the Fossil Plants of the Coal-measures, Part III," 'Phil. Trans.,' B, vol. 186, p. 769, 1895; Potonié, 'Lehrbuch der Pflanzenpalæontologie,' p. 160, 1899; Scott, 'Studies in Fossil Botany,' pp. 307 and 514, 1900.

† G. Wild, "On *Trigonocarpon olivæforme*," 'Manchester Geol. Soc. Trans.,' vol. 26, p. 434, 1900.

‡ Scott, 'Studies' p. 334; Miss Benson, "The Fructification of *Lyginodendron Oldhamium*," 'Ann. of Bot.,' vol. 16, p. 575, 1902.

§ 'Zeiller, 'Éléments de Paléobotanique,' 1900, p. 370.

In general structure the seed *L. Lomaxi* agrees with *L. ovoides*.

It is an orthotropous seed, circular in transverse section, and broadest midway between base and apex. The height of the seed slightly exceeds the diameter, and in general form it may be compared with a Jaffa orange. Its height in full-sized specimens is about 5½ mm., the diameter at the equator 4¼ mm. Many of the specimens that have passed through our hands show signs of having become detached through the agency of a layer of separation and bear a low conical papilla centrally placed at the chalazal end, beneath which the actual layer of abscission was situated.

In the most general relations of its organisation the seed approaches the Gymnosperm type in that the integument and nucellus are distinct from one another in the apical region only, whilst the body of the seed, which contains the large single macrospore with traces of prothallial tissue, shows complete fusion of the integumental and nucellar tissues. But in other respects the seed is remarkable. The free portion of the nucellus which stands above the macrospore is conical and actually engage with broad low grooves on the surface of the wall of the pollen-chamber.

The vascular system of the seed enters as a single supply-bundle at the chalazal papilla, and branches, a little below the base of the macrospore, into nine radially-running bundles. Each of these bundles passes, without further branching, to the apex of the seed, running outside the macrospore and a little distance below the surface. At the canopy the bundles enter the chambers and end at the tips.

Lagenostoma Lomaxi was thus a seed or seed-like structure, detached as a whole and containing pollen-grains in the remarkable cleft-like pollen-chamber; the integument in its free part, when compared with that of Williamson's *Lagenostoma physoides*, suggests a number of originally free arms or processes that have become laterally fused into a complex, chambered organ.

The seed, *L. Lomaxi*, is in some cases still attached to its pedicel;* the great peculiarity of this seed, as compared with other members of the genus, is that when young, and sometimes even at maturity, it is found enclosed in an envelope or cupule, springing from the pedicel just below the base of the seed, and extending above the micropyle—at least in young specimens. The cupule appears to have been ribbed below, and deeply lobed in its upper part; in form it may be roughly compared to the husk of a hazel-nut—of course on a very small scale.

The pedicel and cupule bear numerous capitate glands, of which some are practically sessile, others shortly stalked, while in others again the stalk is of considerable length. The head, or secreting portion of the gland, which is spherical in form, is almost invariably empty, only the multicellular wall persisting. The tissue of the stalk of the gland, consisting of many layers of cells, is preserved, though in a somewhat disorganised state.

These cupular glands present the closest agreement in size, form, and structure with the glands which occur on the vegetative organs of *Lyginodendron Oldhamium*,† and which are especially abundant on the particular form of that plant found in association with *Lagenostoma Lomaxi*. Both on petiole and cupule the majority of the glands are short, those which are not sessile being commonly about 0·4 mm. in height. Long-stalked glands, exceeding a millimetre in height, sometimes occur both on the vegetative organs and on the cupule. The dimensions of the head of the gland agree exactly on cupule and petiole, the diameter averaging about 0·2 mm. in each case. In both the stalk is usually somewhat narrower than the head, except at the base, where it is often considerably enlarged. On the stem, as might be expected, the glands are usually somewhat larger than on petiole or cupule.

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* *Cf.* Williamson, *loc. cit.*, Part VIII, fig. 68 (*L. ovoides*).

† It has long been realised that the name *Lyginodendron* Oldhamium characterises a type rather than a species. It is probable that the very glandular form occurring at Dulesgate may deserve specific rank.

As a rule, the structure of the glands on the vegetative organs is well preserved, the secretory tissue in the head being perfect. But occasionally the vegetative glands are found in the same state of preservation as those on the cupule, with the head hollow, owing to disappearance of the secretory mass. Where we thus have the two organs in a corresponding state of preservation, the agreement between the vegetative glands of *Lyginodendron* and those on the cupule of *Lagenostoma Lomaxi* is found to be exact.

There is no other known plant from the Coal-measures with glands at all similar to those described, nor is it likely that any unknown Gymnosperm should so exactly resemble *Lyginodendron* in these characters. On the ground, then, of the glandular structure we are led to the conclusion that the seed *Lagenostoma Lomaxi* can have belonged to no other plant than *Lyginodendron Oldhamium*, and more particularly to the glandular form of that type with which the seed is associated.

The state of preservation of the glands and of the cupule as a whole, indicates clearly that this organ, as we find it, was in an effete condition, having, no doubt, already discharged its functions while the seed which it protected was still quite young.

The vascular system of the cupule was well developed, and is very so-called "fern-fronds" of the Palæobotanist really belonged to Spermophyta. It is at present impossible to say at what stage in the evolution of the Fern-Cycad phylum, the great change in reproductive methods came, whether it followed in the wake of general anatomical advance, or *vice versâ*. The discovery of further evidence as to the reproductive processes of these ancient plants may be expected to yield interesting results.

The authors are much indebted to Miss Marie Stopes for her valuable aid in the examination of the numerous sections in the Williamson and various other Collections.

Mr. James Lomax deserves high praise for his good judgment and skill in collecting and preparing the material for the investigation.

A full account of the fossils dealt with in the present note is in preparation, and will shortly be submitted to the Royal Society.

"On the Physiological Action of the Poison of the Hydrophidæ."

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It has long been known that the great group of the Hydrophidæ, or Sea-snakes, are poisonous, and cases of death produced by their bites have been recorded, for example, that in Sir Joseph Fayrer's work on the Poisonous Snakes of India, of the ship's captain bitten while bathing in the Bay of Bengal, with a fatal result. The fishermen on this coast are also well aware of the danger of the bites of these reptiles, and take such good care to avoid them, that deaths among them are quite uncommon as far as I can ascertain. Deaths, however, not very rarely occur among those employed in oyster fisheries in shallow water in some places on the Madras coast, owing to snakes being trodden on, so that a study of the nature of the poison of this class of snakes has a practical as well as a scientific side, and, as far as I can gather from the literature of the subject obtainable in Calcutta, it has not yet received much attention. During the last year I have been investigating the subject, and although the amount of poison I have been able to obtain has been very small, yet it has sufficed to allow of certain definite results being obtained, which will be summarised in the following paper.

The Collection of the Poison.

The Hydrophidæ are met with in large numbers all round the coasts of the Indian peninsula, and have been specially studied at Puri on the east coast in Orrisa. It was at this place that I obtained my specimens, which are caught by the fishermen in their nets during the calm cold-weather months with a frequency which is in proportion to the number of fish taken. By small payments they were induced to bring them to a tank which I had constructed near the beach, in which they usually only lived a few days, although some survived several weeks. By making them bite on a watch-glass covered with a thin layer of guttapercha tissue stretched tightly across it, they eject the poison into the glass as clear drops free from all saliva. This is then dried over calcium chloride or strong sulphuric acid, and can then be kept indefinitely in dry well-corked glass tubes, without losing its potency. The snake which is met with in greatest abundance in Puri is the *Enhydrina Bengalensis*, measuring from three to five feet in length, and it has a thick body and a large head. This species also furnishes the largest amount of poison, and from this alone have I yet been able to obtain a sufficient quantity to allow of a considerable number of experiments being performed with it. That of four other species, belonging to three different genera, has also been obtained in small quantities, so that four out of the six genera of Indian Hydrophidæ have now been examined, and will be dealt with.

Appearance and Quantity Ejected.

impossible to get any further poison from it, even if it bite vigorously. Yet if made to bite a small fish immediately after ejecting his poison, the bite is fatal in a short time, showing how fatal a trace of it is. The largest amount of poison obtained at a single bite was 0·023 gramme. The other species mostly gave a smaller quantity than the Enhydrina.

Effect of Heat on the Poison.

On boiling a dilute solution of the poison, it becomes slightly opalescent. After being boiled for 15 seconds, two minimal lethal doses were recovered from, after slight symptoms had appeared, but four minimal lethal doses proved fatal in a somewhat longer time than with unheated poisons. After boiling for one minute, four minimal lethal doses were recovered from after only slight symptoms. Thus the poison is readily destroyed by boiling for a short time, but merely bringing it to the boiling point does not materially affect its strength. Some similar experiments with Cobra poison show that the latter is slightly more resistent to heat than is that of the Enhydrina.

Symptoms Produced by the Poison.

The following symptoms are common to all the species yet tested, no differences having been met with, except with regard to the exact amount of the minimal lethal doses in different animals, which will be dealt with presently. Briefly, the symptoms produced by the poison of the Hydrophidæ may be said to be identical with those caused by cobra venom, with one very important exception, namely, that the former venom has no appreciable action on the blood, which is a marked feature of cobra toxin. In the case of warm-blooded animals, such as rabbits, rats or birds, the symptoms produced by sea-snake poisons are as follows. When minimal lethal, or slightly supra-minimal lethal doses are given subcutaneously, there is always a long period before any symptoms of poisoning occur, the time varying in accordance with the dose from half an hour to several hours, in which respect it resembles Cobra, and differs markedly from Daboia venom. If large doses are given, the symptoms set in much earlier, and in that case death rapidly results. The symptoms are best studied by the use of small doses, when the first thing noticed is that the animal remains quietly in one position, and soon begins to show signs of drowsiness, closing its eyes at intervals. Next it begins to nod its head, but every now and then appears to wake up again and opens its eyes. In the case of birds—in which the symptoms can be best seen—the subject of the experiment next sits down on the floor of the cage, and although it can be made to stand up if disturbed, yet there is now evident commencing muscular weakness, and it can only walk with an unsteady gait. By the time this stage is reached, it will be found that the animal is breathing more deeply than normal, while the number of respirations is also increased to a variable, but often considerable, degree. From this time the picture is one of progressive paralysis, affecting all the muscles of the body, and ending with respiratory convulsions. The animal nods more and more deeply, until the nose or beak touches the floor of the cage, only to be raised again with a jerky motion. It is now unable to stand upright, and the eyes remain closed. The respirations are now very deep and laboured, and in case of birds, the beak is half open, and gaping takes place with every inspiration, while the head is more and more lowered until its vertex instead of the beak rests on the floor, and the animal is unable to raise its head. Very soon after this stage of paralysis is reached, convulsions set in, and the respirations immediately fall very greatly in frequency, while they remain deep in character, although less regularly so than before, some being shallow, so that Cheyne-Stokes breathing is somewhat simulated. The convulsions recur, and soon respiration entirely ceases, but the heart continues beating for some time, usually two or three minutes in the case of warm-blooded animals, after the breathing has entirely ceased. When the convulsions commence, the animal rolls over on its side in a state of nearly complete paralysis. Every word of the above description of the symptoms pro-lasting for several hours. Sometimes temporary excitement with rapid motion may be observed for a short time, but more often the picture is simply one of slowly progressing paralysis. In most kinds of fish this is also very well shown by a gradually increasing difficulty in maintaining the upright position, the fish slowly turning over on one side and then swimming up into its upright position again, only to slowly sink on to its side once more. The respirations will now be found to be deeper than normal, although not as a rule quicker, but, on the contrary, they steadily slow down from the beginning of the symptoms to the end without any marked increase in the rate. This paralysis of all the muscles and of the respirations steadily progresses until convulsions set in, to be immediately followed by a very rapid failure of the respirations both in number and depth, so that they become difficult to detect, and death soon follows. The heart will be found beating some time after the breathing has ceased, and no extravasation of blood or other noteworthy change is found *post mortem*. Here again the symptoms are precisely similar in poisoning of fish by Cobra venom.

The Potency of the Poison.

By working out the smallest fatal doses of the poison per kilogramme of weight in different animals, and comparing them with those obtained by former workers for other snake venoms, we shall be able to estimate the potency of that now being dealt with. This has been done in the case of the poison of the Enhydrina by means of numerous experiments carried out with the mixed dried venom of a number of these snakes, with the following results. At the same time comparative experiments were also carried out with fresh dried Cobra venom for comparative purposes. White rats were first tested, and 0·07 milligramme per kilo weight was found to prove fatal, but smaller doses were sometimes recovered from. In the case of Cobra poison 0·5 milligramme per kilo was necessary to produce death, while Lamb in Bombay found the fatal dose of this poison for rats to be 0·33 milligramme. It is evident then that the poison of the Enhydrina is several times as potent as is Cobra venom on rats. In the case of rabbits only a few experiments have been performed, but 0·04 milligramme per kilo proved fatal in under four hours in one case, while in another 0·01 milligramme per kilo produced no symptoms but loss of appetite; but on giving a second dose of 0·02 milligramme per kilo five days later (the animal having fully recovered from the first dose in one day), death resulted in a few hours. On the other hand, Elliot found the minimal lethal dose of Cobra venom for rabbits to be 0·7 milligramme per kilo weight, so that it is evident that these animals are many times as suscep-

ible to the poison of Enhydrina as to that of Cobra, the former
oison being some twenty times as potent for them as the latter—
remarkable difference.

A larger number of experiments have been carried out with
irds, pigeons and fowls. These also bear out the former ones in
roving the far greater potency of the poison of the Enhydrina
ver that of the Cobra or other poisonous snake yet examined. In
he case of pigeons the minimal lethal dose, 0·05 milligramme per
ilogramme, always proves fatal, while in fowls the fatal dose is 0·04.
hese figures may be compared with those obtained by D. D. Cun-
ingham in his numerous experiments with Cobra venom on fowls,
or which he found the minimal lethal dose to be 0·5 milligramme
er kilo, so that the poison of the Enhydrina for birds is at least ten
imes as potent as is Cobra venom, which goes far towards neutralising
he effect of the much smaller dose of poison ejected by the Enhy-
rina as compared with the Cobra. Taking the minimal lethal dose of
he Enhydrina for warm-blooded animals as 0·05 milligramme per kilo,
he fatal dose for an average man of 70 kilogrammes would be
·5 milligrammes, or about one-third of the average amount of venom
jected by a fresh full-grown specimen of this, by far the most com-
nonly met with, kind of snake in the Bay of Bengal. There is good
round, then, for the belief in the deadliness of the Hydrophidæ.

kill a fish as is sufficient to kill a warm-blooded animal—a very marked difference. On testing the same species of fish with the poison of the Enhydrina, it was found that 0·5 milligramme per kilo of freshly dissolved poison was always fatal, and sometimes a smaller dose caused death. Thus the dose of this sea-snake poison required to kill fish was but ten times as much as the minimal fatal dose for warm-blooded animals, that is, considerably less than we found to be the case with Cobra poison. In other words, the poison of the Enhydrina is much more deadly than is Cobra venom for fish, even allowing for the greater potency of the former for warm-blooded animals, so that it appears to be specially adapted for the needs of the Sea-snake, which lives on fish, being in all about fifty times as potent for fish as is Cobra venom. This great concentration of the poison may be of considerable advantage to the reptile when dealing with such active prey as fish in their own element. This special affinity of the poison for fish was even more marked in the case of some of the other species tested. Thus, that of a single species of the *Disteira cyanocincta* was fatal to pigeons in doses of 0·5 milligramme per kilo, being thus considerably weaker than that of the Enhydrina, but only 1 milligramme per kilo was required to kill fish, that is but twice as much as was needed to kill birds. Similarly with the *Disteira viperina* the minimal lethal dose for pigeons was 0·5 milligramme, and for fish only 0·75, or but very little more. Again, the poison of the *Hydrophis cantoris* for both pigeons and fish was just the same as the last-mentioned species. Lastly, the poison of the *Hydrus platurus* killed pigeons in doses of 0·075 and fish in one of 0·25 milligramme per kilo, being thus very deadly for both cold and warm blooded animals. The above include four out of the six genera of Hydrophidæ found in Indian waters, so that, although the poison obtained from the last four species was from single specimens, and therefore cannot be taken as more than approximately accurate, yet they suffice to prove that the Hydrophidæ as a class secrete very virulent poisons, which are specially poisonous to fish. It is also worthy of note that the two genera which proved to be most deadly to warm-blooded animals, namely, the Enhydrina and the *Hydrus platurus*, are just the two which the fishermen at Puri said were the most dangerous ones, as the accuracy of their statement points to actual experience in the human subject of their deadliness having been handed down among them. Some of the smaller species, however, probably do not eject sufficient poison to prove fatal, to adults at any rate, and hence are not so much dreaded by the fishermen. It will also be observed that the poison of the *Enhydrina Bengalensis* is the most potent of those so far tested, while it also yields the greatest amount of poison, with the exception, perhaps, of the *Disteira cyanocincta*.

Effect of the Poison on other Cold-blooded Animals.

I have not yet been able to test any extensive series of other cold-blooded animals to see if they are equally susceptible to the poison of the Hydrophidæ as fish are, but in one instance a frog weighing 30 grammes was injected with a dose of 0·2 milligramme per kilo, with the result that it showed well-marked symptoms of paralysis, but eventually recovered, so that it would appear to have been about as susceptible as fish. Some harmless snakes were injected with noteworthy results. Thus, two specimens of the *Coluber fasciolatus* were injected with doses of 10 and 50 grammes per kilo respectively, with the poison of the Enhydrina, with no ill effect, and the former received a second dose of 50 milligrammes per kilo three days after the smaller dose, equally without effect. Here we have a harmless colubrine snake withstanding 100 times the fatal dose for a fish and 1000 times that for a warm-blooded animal. Further, two specimens of the harmless green Whip-snake (*Dryophis mycterizans*) were tested, but in this species 25 milligrammes per kilo in one instance, and 15 in the other, each produced death in less than two hours, so that a smaller dose would nearly certainly have been fatal. This opens up a large question which must await further investigation.

but without the addition of the venom, used as controls. These control solutions showed no dissolution of the red corpuscles after twenty-four hours. From 5 to 10 cubic centigrammes of blood were added to from ½ to 1 c.c. of the isotonic solution of the poison, varying strengths of the latter being tested in this way. Pigeon's blood is specially well suited for these experiments, as the bodies of the corpuscles are dissolved while the nuclei remain visible. It was found that a 1-in-1000 solution of Cobra venom (1 milligramme in 1 c.c.) produced a very rapid solution of the red corpuscles, which had all disappeared in seven minutes. A 1-in-20,000 solution took a much longer time to produce complete dissolution, namely two and a half hours. In the case of human blood a 1-in-10,000 solution of cobra venom dissolved the whole of the red corpuscles in from fifteen to thirty minutes, while one of a strength of 1 in 20,000 took about one hour to do so. A 1-in-100,000 solution had very much less effect, having produced only a slight diminution in the number of the red corpuscles within one hour's time. The white corpuscles were not dissolved by the venom in the strengths used.

Let us now compare these data with those obtained with the poison of the Enhydrina, bearing in mind the much greater potency of the latter as compared with Cobra venom. The poison of the Enhydrina was mixed in the same way as above described with the blood of pigeons and with human blood, in strengths of 1 in 1000, with the result that at the end of one or two hours there had been no appreciable dissolution of the red corpuscles. On testing again several hours later, slight dissolution was found to have taken place, and by this time the solution also showed naked-eye evidence of commencing hæmolysis. After having been kept at room temperature (from 70° to 80° F.) for twenty-four hours the dissolution appeared to be complete, but, on examination with the microscope, a few red corpuscles were still found to be undissolved, showing that even after this lapse of time the hæmolytic change was not quite complete. The poison of the *Disteira cyanocincta* and the *Hydrophis cantoris* were also tested in the same way with precisely similar results, namely, that a strength of 1 in 1000 had no appreciable hæmolytic effect at the end of one hour, but caused nearly complete dissolution at the end of the course of twenty-four hours. This is about the same effect as is brought about by a solution of Cobra venom of a strength of 1 in 100,000, although Cobra venom has a potency of only one-tenth that of the poison of the Enhydrina. Thus we find that in proportion to its potency the poison of the Cobra has about 1000 times as great a hæmolytic effect on the red corpuscles of warm-blooded animals as has that of the Enhydrina. We have already seen that the latter poison produces no blood-stained effusion at the site of *the injection of a fatal* dose, evidently on account of the strengths used aving no hæmolytic action, for the solutions employed for the small nimals experimented on were 1 in 10,000 or less. If we work out he amount of poison required to dissolve a certain amount of the lood of a pigeon, for example, we find that it takes about 200 times fatal dose to dissolve 1/2000th part of the bird's blood in twenty- our hours, calculating this fluid to be one-thirteenth of its body eight. It is obvious, then, that ordinary fatal doses of the poison of he Hydrophidæ can have no appreciable hæmolytic effect, and that eath cannot be attributed, even in a partial degree, to its action on he blood of the animal killed by it. This can also be demonstrated y another method of experiment, namely, by counting the number of he red corpuscles before the administration of the fatal dose of he poison, and again immediately after death. This I have done everal times, with the result of showing that no dissolution of the red orpuscles resulted from the action of a lethal dose of the Enhydrina oison. For example, a fowl's blood was counted, and 3,190,000 red orpuscles per cubic millimetre were found. A lethal dose of Enhy- rina poison was then injected subcutaneously, which proved fatal in ust one hour, when the blood count showed 3,120,000 red corpuscles n the same quantity of blood.

Next we have to deal with the action of the poison on the coagul- bility of the blood. In the case of Cobra venom marked changes are destroyed the clotting power, and this, too, it must be remembered, in spite of the Enhydrina poison being ten times as powerful as that of the Cobra. It is evident, therefore, that the poison of the Enhydrina has no appreciable effect in ordinary dilute minimal lethal doses on the coagulability of the blood, while, as a matter of fact, we have already seen that such doses do not produce any loss of the clotting power of the blood. This was also the case when fifty times a minimal lethal dose of the venom was injected into the vessels of rabbits with the result of causing death in about six minutes.

The above experiments show that the poison of the Hydrophidæ has no appreciable action on the blood of animals, which can in any way account for the symptoms and fatality caused by it, yet it kills with precisely the same symptoms as are produced by Cobra venom, and, as we shall see presently, there are good reasons for believing that it has a special action on the nervous system. It will be evident at once that this furnishes a very strong argument in favour of the view that Cobra venom also kills through the nervous system, as held by Lauder Brunton and Fayrer, and not through the blood, as maintained by Cunningham. It is also of special interest to observe that although the action of the poison of the Hydrophidæ on the blood is practically a negligible quantity in its lethal effects, yet it still persists to a slight, but easily demonstrable, degree; for if it so persists in the Sea-snake, it may also persist in a still greater degree in the case of the Cobra without being a very active agent in the lethal effects produced by that poison, which kills through the nervous system as does that of the Hydrophidæ. In this connection it is interesting to observe that all through the poisonous snakes we find evidence of an action on the blood and on the nervous system in different degrees. Thus, beginning with the Viperine snakes, we first have the *Vipera Russellii*, which appears to be the purest blood poison of the known venomous snakes, killing by producing intravascular clotting in large doses, and the opposite effect of total loss of coagulability in repeated sub-minimal lethal ones. Then we come to the class of Pit-vipers, of which the rattlesnake of America has been most closely investigated by Weir Mitchell and Reichert. They also found a very marked effect on the blood, apparently similar to that produced by the Daboia, but, combined with this, we have a marked paralytic effect on the nervous system, and especially on the respiratory centre, for the authors mentioned conclude that although death may occur through the effect on the blood, yet they add "There can be no question, however, that the respiratory centres are the parts of the nervous systems most vulnerable to the poison, and that death is commonly due to their paralysis." Leaving the Viperine snakes and passing on to the poisonous Colubrines, we first come to the Australian species, so ably studied by C. J. Martin, namely, the *Pseudechis,* and we find again a combination of the two effects to such a marked degree that, when the venom is administered intravenously, death results from intravascular clotting, as in the Viperine snakes, while if minimal lethal doses are given subcutaneously death results through paralysis of the respiratory centres. Next we come to the Cobra, another Colubrine snake, and here we find the nerve symptoms quite predominate, although some considerable effect on the blood in the form of reduction of coagulability and dissolution of the red corpuscles still survives, although it now takes quite a secondary position to the effect on the nervous system. Lastly, we have the Hydrophidæ, which, morphologically considered, are but colubrines modified for an aquatic existence, and here we find a practically pure nervous poison, although there still persists a trace of action on the blood if strong solutions of the venom are employed, although it can have no actively poisonous effect. The very slight action found, however, may be of some value to the snake in the following way. We have seen that a 1-in-200 solution of the Enhydrina poison has a slight retarding effect on the clotting power of the blood, which would doubtless be more marked in still more concentrated solutions, so that it is highly probable that the pure poison would have the effect of preventing the clotting of the blood at the point of injection of the poison, and so allow of its more ready absorption into the circulatory system through the patent vessels severed by the fang. This will the cessation of the heart's beat. The effect on the respiration was simply a uniformly steady slowing down until convulsions set in, when the breathing finally ceased at once. For example, in a rabbit which had received a dose of 1 milligramme per kilo. directly into the carotid artery (the artery being clamped immediately afterwards to prevent hæmorrhage), the respirations were 60 per minute immediately before the injection of the poison. During the four minutes immediately following the injection, the number of respirations were as follows:—first minute, 56; second minute, 51; third minute, 42, and the fourth minute 33. In the first quarter of the 5th minute they were 8, at which point convulsions set in and the breathing stopped. The respirations were written down every quarter of a minute, and the figures for the separate quarters show an equally steady diminution of the number of respirations as the minute periods just given. In the same experiment the pulse showed the following changes. Before the injection it was 105 per minute. During the second half of the first minute after the injection it was 47 (that of the first half minute was lost), during the second minute it was 106, showing no alteration up to this time. During the third minute it fell to 99, and during the fourth it further fell to 48, that for its first half having been 32, and for the second half 16. During this steady fall in the pulse rate, its volume and force became increased. During the fourth minute, as already mentioned, convulsions set in, and the pulse was lost for about a minute, only the first and third quarters of the fifth minute having been recorded as 8 and 11 beats respectively. During the last three quarters of the sixth minute the beats were 15, 15 and 17 respectively, being now very feeble instead of unusually full, as before the cessation of respiration and onset of convulsions. During the first and second quarters of the seventh minute, the beats were 26 and 20 respectively, at which point the heart finally ceased to beat, that is, three and a half minutes after the cessation of the breathing. Very similar results have been obtained in another experiment, in which the same dose was injected into the jugular vein, a steady fall in the respirations first occurring, and they ceased with the onset of convulsions, while an equally steady fall in the pulse rate occurred later than that of the respirations, accompanied with an increased volume of the artery, the tension rapidly falling when the respiratory convulsions set in, but the pulse at the same time became more rapid again until it finally declined once more and then ceased. These experiments appear to show that the primary effect of the poison is a paralysing action on the respiratory centre, and that the cardiac failure is secondary to that of the respirations. The exact explanation of the slowing of the pulse with increased volume of the artery, I am not prepared to say without the aid of pressure tracings, which I have not yet been able to take.

The Affinity of the Nervous System for the Poison.

We have seen that the poison of the Enhydrina is much more potent than even that of Cobra, and it appears to be somewhere intermediate in virulence between Cobra and tetanus toxins. Further, we know that the repeated injection of gradually increasing doses of the latter two poisons into susceptible animals leads to the formation of an antitoxin in the system. This marked similarity of the nerve poisons of the Colubrine Snakes and tetanus toxin leads one to inquire whether these snake venoms do not exert their noxious influences in the same way that tetanus toxin does, namely, by being taken up from the circulation and fixed in the nerve cells until a sufficient dose has been absorbed to paralyse the nervous matter. We know from the experiments of Wassermann that small amounts of tetanus toxin can be thus fixed by fresh nerve matter in a test-tube, and so rendered inert when subsequently injected into a susceptible animal. It seemed to be worth while to repeat these experiments with the poison of the Enhydrina, and although I have not had time to carry out a sufficiently exhaustive series of experiments to settle this point, yet the following data appear to me to have some value as being highly suggestive of the mode of action of these nerve-paralysing snake venoms.

ments, then, point to the action of the toxins of the Enhydrina being very similar in nature to that produced by the tetanus bacillus. A few experiments were also done with Cobra poison in the same way, using the cerebrum only, but here the results were not so marked as in the case of the Enhydrina poison, only a retardation of the onset of symptoms and of death having been observed.

Antitoxins.

Lastly, we have to deal with the question of the possibility of obtaining an antitoxin against the poison of the Hydrophidæ. It has now been abundantly proved that Calmette's antivenin is not a specific against all kinds of snake venom, as he claimed, although in large doses (40 c.c. according to Lamb) it is undoubtedly of great value against the poison of the Cobra. The very marked similarity of the symptoms of poisoning by the Hydrophidæ with that produced by Cobra, lead one to hope that the antitoxin, which is efficient against the latter, would also be of value against Sea-snake venom. This has been put to the test by adding minimal and slightly supraminimal lethal doses of the poison of the Enhydrina to one half c.c. of fresh Calmette's antivenin (which had only reached Calcutta a very short time before it was used), and after allowing the mixture to stand at blood heat for half an hour, injecting the whole subcutaneously. White rats were used in the experiments, and the amount of antivenin in proportion to the amount of poison was relatively enormous as compared with the dose recommended in the treatment of men bitten by venomous snakes. Yet the animals uniformly died in just about the same time as the controls, so that it is evident that Calmette's serum is of no use against the poison of the Hydrophidæ.

On the other hand, the similarity in the action of this poison to the Cobra and tetanus toxins leads one to expect that an antidote could be prepared against it in a similar way to those of the latter poisons. It is only during three months that I have been able to experiment on this point, fowls being used. It soon appeared that the doses had to be very slowly increased, or fatalities occurred, and in the limited time these experiments lasted, I was only able to immunise one fowl against the minimal lethal dose of this poison, and a slightly larger dose proved fatal with the usual symptoms. My intention was to immunise a series of animals against the Enhydrina poison, and then to test them with small doses of poisons from the other Hydrophidæ, as owing to the large variety of this class of snakes, no antidote would be of any practical value unless it was equally potent against all the genera and species, or at least against the ones most commonly met with. This important and interesting question must await further investigation.

One experiment, which was carried out in order to test if the serum or bile of the Enhydrina had any antidotal properties, deserves mention in this connection. Three puppies of the same litter were used, all very much of the same size. Each received an equal quantity of Enhydrina poison, but in the first this was mixed with a four minims of the serum of the Enhydrina; in the second it was mixed with four minims of the bile of the same snake; and the third received the poison solution only as a control. The mixtures were injected ten minutes after being made. The result was that all three animals died in a little over an hour, the control surviving slightly longer than the others. It appears, then, that neither the serum nor the bile of this snake has any antidotal properties against the poison, and can not, therefore, be utilised in the treatment of their bites. Further research will be necessary to determine if a practically efficient antidote can be prepared, which I hope to undertake when sufficient venom for the purpose can be obtained.

This concludes the most important experiments so far carried out by me with the poison of the Hydrophidæ. They have necessarily been strictly limited by the very small amount of poison which I have yet been able to obtain, and by the equipment of the laboratory at my disposal, for the use of which I am indebted to the kind permission of the Committee of the Zoological Gardens of Calcutta. I am also indebted to the Bengal Government for a grant towards the expenses of

"Experiments in Hybridisation, with Special Reference to the Effect of Conditions on Dominance." By L. DONCASTER, B.A., King's College, Cambridge. Communicated by Dr. S. F HARMER, F.R.S. Received March 19,—Read May 7, 1903.

(Abstract.)

The paper describes experiments made at Naples with hybrid Echinoid larvæ. The object of the experiments was to determine whether the dominance of a character is influenced by the condition of the genital cells at the time of fertilisation. It had been suggested by Vernon* that the "prepotency" of the sexual cells varies with their maturity, and experiments were made to test this conclusion, and also to discover whether "prepotency" could be influenced by other conditions acting on the eggs or spermatozoa before fertilisation.

It was found that adverse conditions acting on the eggs did give rise to differences in the larvæ, but evidence is given to show that these differences are not due to a change in the dominance of characters, but are the result of differences in the vigour of the larvæ. It is also shown that the seasonal changes observed by Vernon are probably due chiefly, if not entirely, to differences of temperature, and are not caused by a change of dominance accompanying difference of maturity.

It is also shown that if an individual, A, shows greater dominance than B when each is crossed with a specimen X of the other sex, then A will also show greater dominance than B when both are crossed with a specimen Y.

It is shown that the different characters of one parent are inherited separately by the hybrid offspring, so that there is no pronounced correlation in the offspring between characters derived from the same parent. Further, a given character may appear in very different degrees on the two sides of the body of a hybrid larva, so that the hybrids are very frequently asymmetrical, although in the characters considered the two sides of the pure-bred larvæ are similar.

Experiments are described dealing with the causes which hinder cross-fertilisation between separate species, and it is shown that treatment of the eggs which tends to reduce their vitality usually renders their fertilisation by sperm of another species more easy.

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* 'Phil. Trans.,' B, 1898.

"Preliminary Note on the Discovery of a Pigmy Elephant in the Pleistocene of Cyprus." By DOROTHY M. A. BATE. Communicated by HENRY WOODWARD, LL.D., F.R.S., F.G.S., V.P.Z.S., late Keeper of Geology, British Museum, Natural History. Received April 23,—Read May 7, 1903.

While still in Cyprus the receipt of a grant from the Royal Society in April, 1902, enabled me to devote a considerable amount of time not only to making more extensive excavations in some of the caves previously found, but also to a search for further cave deposits. I confined my attention chiefly to the Keryina range of limestone hills in the north of the island in the hope of finding bone caves containing other remains than those of the pigmy *Hippopotamus*, of which Dr. Forsyth Major has already given a short description* from specimens discovered by myself.

In this search I was at length successful, although it was not until a certain amount of tentative digging had been carried on in four out of five newly discovered deposits that work was started on what appeared at first to be the most unpromising looking place which had been found, and was consequently the last to receive attention.

However during the first day one of the workmen found, not far dwarf species from the Pleistocene deposits of the caves and fissures of Malta and Sicily. It was thought probable that they would differ from these; the fact of the pigmy hippopotamus of Cyprus being distinct from those found in the other large Mediterranean islands, lending colour to the supposition; this expectation was fulfilled, for the Cyprus fossils do not appear to be identical with any of the Maltese species, though they seem to come nearest to *Elephas melitensis* both in size and in the number of plates in the molars. The number of these plates in any particular tooth is liable to vary to a certain extent, but on taking the average, as far as this can be judged from the amount of material available, the resulting ridge formula, exclusive of talons, is $\div \frac{5}{5}\ \frac{7\text{–}8}{7\text{–}8}\ \frac{8}{8}\ \frac{9}{9}\ \frac{12}{12}$ which practically agrees with that of *E. melitensis* given by Dr. Falconer.[*]

The teeth of the Cypriote elephant are considerably smaller than those of *E. mnaidriensis*, from both Sicily and Malta, this being the largest species from the last-named island. They also differ somewhat in their ridge formula, which is that mentioned above, while Dr. Leith Adams[†] gives that of *E. mnaidriensis* as $\frac{3}{3}\ \frac{6}{6}\ \frac{8\text{–}9}{8\text{–}9}\ \frac{8\text{–}9}{8\text{–}9}\ \frac{10}{10}\ \frac{12\text{–}13}{12\text{–}13}$.

The Cyprus form seems to have been also slightly inferior in size to *E. melitensis*, for its largest upper and lower molars do not equal, either in length or breadth, some of the specimens of the corresponding teeth of this Maltese species which are in the collection of the British Museum. Its tusks differ from all those from Malta in being compressed laterally, which character is especially noticeable in those of the female and young; further, they appear to be more strongly curved than those of *E. melitensis*.

As a general feature it may be said that the molars from Cyprus are, on the whole, more simply constructed than those of *E. melitensis*. They show a still slighter tendency to "crimping" in the bands of enamel, and are less inclined to develop the mesial expansion of the plates of dentine which is not uncommonly found in the teeth of *E. melitensis*, and is so conspicuous in those of *E. Africanus*.

It is well known that when the plates of an elephant's tooth first come into use, the edging of enamel is in the form of a series of rings owing to the digitation of the plates. These are later worn into a single band surrounding the enclosed area of dentine.

In the Maltese specimens it is not uncommon to find the encircling enamel persisting thus divided for a considerable time. Even four or five ridges may remain in this condition at one time in a single tooth, with perhaps an anteriorly decreasing number of rings. This is well shown in a tooth, now in the British Museum collection, doubtfully ascribed by Mr. Busk[*] to the first upper true molar of *E. Falconeri*. This is not so much the case in the Cyprus specimens, in which the bands of enamel only remain thus separated into several annuli for a very short while after the plate comes into wear.

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[*] 'Pal. Mem.,' vol. 2, p. 298. London, 1868.

[†] 'Zool. Soc. Trans.,' vol. 9, 1874, p. 112.

The molars vary considerably, some specimens having very broad crowns while others are somewhat narrow. The bands of cement are wide, in perhaps the majority of cases almost, or quite, equalling in width the plates of dentine; this seems to be the exception and not the rule in the molars of *E. melitensis*.

Taking into consideration the several characters in which the teeth of the Cyprus elephant differ from those of all the hitherto described dwarf species (putting on one side *E. lamarmoræ*[†] from the Pleistocene of Sardinia, the teeth of which are unknown to science) as well as the distinct habitat of the animal, I have come to the conclusion that it is specifically distinct from these other small forms, though possibly they were derived from a common ancestor, and I, therefore, propose to name it *Elephas cypriotes*.

The discovery of the remains of this pigmy elephant, as well as of *Hippopotamus minutus*, in Cyprus, is interesting in comparison with the dwarf species from Malta and Sicily, and because the presence

"On the Discovery of a Species of Trypanosoma in the Cerebro-Spinal Fluid of Cases of Sleeping Sickness." By Aldo Castellani, M.D. Communicated by the Malaria Committee of the Royal Society. Dated "Entebbe, Uganda, 5th April, 1903." Received May 8,—Read May 14, 1903.

On the 12th November, 1902, when examining a specimen of cerebro-spinal fluid taken by lumbar puncture during life from a well-marked case of *sleeping sickness*, I was surprised to observe a living trypanosoma. Since that date I have made as many observations in this direction as possible, and the results are to my mind sufficiently surprising to excuse me for presenting this preliminary note.

These trypanosomes are not in large numbers, so that to find them it is necessary to draw off at least 15 c.cs. of the cerebro-spinal fluid. It is better to reject the first few c.cs. as they are apt to contain blood. When the fluid comes away clear, 10 c.cs. are collected and centrifuged for 15 minutes. At the end of this time there is found at the bottom of the tube a slight deposit of whitish sediment, and in some cases also a minute trace of blood.

The liquid above the sediment is poured off and the sediment examined under a moderately low power of the microscope. As the trypanosomes are at first fairly active they are easily detected.

The following tables represent the results of this investigation :—

Sickness Cases.

icroscopic earance of ediment.	Presence of trypanosoma in cerebro-spinal fluid.	Remarks.
leucocytes, the rity of which are nuclear. Some rare red blood scles	Present	
few leucocytes .B.C.	Absent	
eucocytes. No .	Absent	
ucocytes. Very .B.C.	Present	Patient died on December 18, 1902. No complications. In fluid from lat. vent. Tryp. present.
ucocytes. Very .B.C.	Absent	
leucocytes and R.B.C.	Absent	
eucocytes. No .	Present	In fresh preparations of blood taken from a finger the same day I found a few trypanosomes apparently similar to those found in the lumbar puncture liquid, only their movements were apparently more lively.
eucocytes and R.B.C.	Absent	
leucocyt s and few R.B.C.	Absent	

Table I —Sleeping Sickness Cases—*continued.*

No.	Name.	Sex.	Age.	Date.	Stage of disease.	Microscopic appearance of sediment.	Presence of trypanosoma in cerebro-spinal fluid.	Remarks.
9	Makassa	F.	25	25/1/03	3rd	Some leucocytes. No R.B.C.	Present	
10	Kaperi II	M.	14	25/1/03	2nd	A few leucocytes and very few R.B.C.	Absent	
11	Ally II	M.	30	2/2/03	3rd	Some leucocytes and R.B.C.	Absent	
12	Mocreza	M.	30	10/2/03	2nd	A few leucocytes and R.B.C.	Absent	
13	Budara	M.	22	27/2/03	2nd	Some leucocytes and very few R.B.C.	Present	
				2/3/03	..	Some leucocytes and very few R.B C.	Present	
14	Nombi	F.	30	27/2/03	1st	Few leucocytes and R.B.C.	Absent	
				24/3/03	2nd	Some leucocytes and R.B.C.	Absent	
15	Fatoma	F.	18	27/2/03	2nd	Very scarce leucocytes and no R.B.C.	Absent	
				4/3/03	..	Few leucocytes and R.B.C.	Absent	
				26/3/03	..	Few leucocytes and R.B.C.	Absent	
				1/4/03	..	Some leucocytes and a few R.B.C.	Present	
16	Zenabu	F.	22	24/3/03	1st	No R.B.C.	Absent	
				28/3/03	1st	Some R.B.C.	Absent	
				1/4/03	1st	Some R.B.C.	Absent	

Table I.—Sleeping Sickness Cases—*continued.*

No.	Name.	Sex.	Age.	Date.	Stage of disease.	Microscopic appearance of sediment.	Presence of trypanosoma in cerebro-spinal fluid.	Remarks.
17	Benjamin	M.	20	25/3/03	2nd	..	Absent	
				28/3/03	..	Some few leucocytes and R.B.C.	Absent	
				29/3/03	..	Some few leucocytes and R.B.C.	Absent	
18	Zakibu	M.	25	25/3/03	2nd	Few leucocytes and R.B.C.	Present	
				27/3/03	2nd	Few leucocytes and R.B.C.	Present	
19	Seera	M.	25	26/3/03	2nd	..	Present	
20	Kimbra..........	M.	55	26/3/03	3rd	Some leucocytes. No R.B.C.	Present	
21	Abdulla	M.	..	26/3/03		No R.B.C.	Present	
22	Kagoya..........	F.	20	26/3/03	3rd	No R.B.C.	Present	
23	Keogaffum	M.	55	27/3/03	2nd	Some leucocytes. No R.B.C.	Absent	
				1/4/03		..	Absent	
24	Jacobo	M.	20	28/3/03	3rd	Some leucocytes. No R.B.C.	Present	The trypanosomes were much more numerous than in other cases.
25	Iegobaza	M.	40	27/3/03	2nd	Few leucocytes. No R.B.C.	Present	
26	Ibsarara..........	F.	35	27/3/03	3rd	Some leucocytes. No R.B.C.	Present	
27	Leobeni	M.	25	28/3/03	2nd	No. R.B.C.	Present	
28	Kidorme	M.	20	28/3/03	2nd	Some leucocytes and R.B.C.	Present	

Table I.—Sleeping Sickness Cases—*continued.*

No.	Name.	Sex.	Age.	Date.	Stage of disease.	Microscopic appearance of sediment.	Presence of trypanosoma in cerebro-spinal fluid.	Remarks.
29	Kcagabidoia	M.	55	28/3/03	3rd	Some leucocytes and R.B.C.	Absent	
				1/4/03	3rd	Some leucocytes and R.B.C.	Absent	
30	Kitaroma	M.	25	28/3/03	2nd	Some leucocytes and R.B.C.	Absent	Only 5 c.c. of liquid collected.
				2/4/03	2nd	Some leucocytes and R.B.C.	Absent	
31	Waiswa	M.	10	29/3/03	2nd	Some leucocytes and R.B.C.	Present	
32	Kaperi I.	M.	8	23/3/03	3rd	No. R.B.C.	Present	Trypanosoma present also in the fluid taken from lateral vent. at the *post-mortem*.
33	Matasa	M.	28	29/3/03	2nd	Few leucocytes and R.B.C.	Absent	
				2/4/03	2nd	Few leucocytes and R.B.C.	Absent	
34	Decodeno	M.	25	31/3/03	2nd	Few leucocytes and R.B.C.	Present	

ontrols.

Microscopic appearance of sediment.	Presence of trypanosoma.	Remarks.
Very few leucocytes and R.B.C.	Absent	
Very few leucocytes and R.B.C.	Absent	Dr. Baker found *Trypanosoma Gambiense* in blood some days ago.
Few leucocytes. No R.B.C.	Absent	
Few leucocytes and R.B.C.	Absent	Dr. Baker found *Trypanosoma Gambiense* in blood the same morning.
Few leucocytes. No R.B.C.	Absent	
Very few leucocytes and R.B.C.	Absent	
Very few leucocytes and R.B.C.	Absent	
Very few leucocytes and R.B.C.	Absent	
Very few leucocytes and R.B.C.	Absent	
Very few leucocytes and R.B.C.	Absent	
Very few leucocytes and R.B.C.	Absent	
Very few leucocytes and R.B.C.	Absent	Dr. Baker found *Trypanosoma Gambiense* in blood the same morning.

Table I shows that in 34 cases of sleeping sickness, the trypanosomes were found in the cerebro-spinal fluid taken by lumbar puncture during life in 20 cases, giving a rate of 70 per cent.

On two occasions I also examined in the same way fluid from the lateral ventricles and in both cases found the same parasite. In blood I found the trypanosoma once with certainty.

It may be thought that the trypanosomes are found in the cerebro-spinal fluid on account of the trace of blood which sometimes forms part of the sediment. But it will be seen from the table that in several cases there was no trace of blood.

Table II shows that in 12 cases of ordinary disease, the cerebro-spinal fluid taken during life by lumbar puncture, in no case contained trypanosoma, and it is important to note that 3 of these controls were cases of the usual trypanosoma fever, as described by Forde, Dutton, Manson, Daniels, &c.

Here it may be remarked that trypanosoma fever is by no means uncommon among the natives in Uganda, 3 cases having been met with, by Dr. Baker, one of the colonial surgeons here (Entebbe), within the last 3 weeks. I understand that Dr. Baker is publishing this most interesting observation. It must be clearly understood that these cases of trypanosoma fever bear no resemblance in their clinical features to sleeping sickness.

The trypanosoma found in the cerebro-spinal fluid of sleeping sickness does not as far as I have been able to make out differ materially in size and shape from the species one finds in the blood of trypanosoma fever, *Trypanosoma Gambiense* (Dutton), but possibly it is to be differentiated from this one, because in it, as a rule, the micro-nucleus lies nearer the extremity and the vacuole is apparently larger. Besides, its movements are not apparently so active, but this fact might be due to the effects of the centrifuge. In case it should prove to be a new species, the trypanosoma I have described might be called from the country where I have found it first—*Trypanosoma Ugandense.*

Relation of the Trypanosoma to Sleeping Sickness.

At the post-mortem examination of 80 per cent. of the cases where I found during life the trypanosoma, I grew from the blood of the heart and from the liquid of the lateral ventricles the variety of streptococcus I described many months ago in my first note. Up to that time I had never found the trypanosoma, but this is easily explained by the fact that I did not use the technique I have described in this note, viz., examination of a *large* quantity of liquid after long use of the centrifuge.

Influenced by my last investigations I would suggest as a working hypothesis on which to base further investigation that sleeping sickness is due to the species of trypanosoma I have found in the cerebro-spinal fluid of the patients in this disease, and that at least in the last stages there is a concomitant streptococcus infection which plays a certain part in the course of the disease.

Note by the Secretary of the Royal Society.

As so far supporting the observations by Dr. Castellani recorded i the above communication, it may be desirable to state that Colonel Bruce, to whom in Uganda Dr. Castellani made known his discovery of the Trypanosoma, and who is now continuing the investigation begun by Dr. Castellani, has sent to the Royal Society a telegram, received May 4, stating that since Dr. Castellani left, in thirty-eight cases of sleeping sickness, he had found trypanosoma in every case in fluid obtained by lumbar puncture, and that he had found trypanosoma in the blood in twelve out of thirteen cases of sleeping sickness.

MICHAEL FOSTER.

"On Skew Refraction through a Lens; and on the Hollow Pencil given by an Annulus of a very Obliquely Placed Lens."

By J. D. EVERETT, F.R.S.

Received January 22,—Read January 29, 1903. Received in revised form, April 20, 1903.

[PLATES 9 AND 10.]

PART I.—*Outline.*

1. The investigation here described was undertaken with the view of explaining the curious curves obtained by receiving on a screen, at certain distances, the hollow pencil which emerges from an annulus of a lens placed at large obliquity (such as 30° or 45°) to the incident beam.*

2. The first requisite is a process for calculating the direction-cosines of a ray refracted at a given surface, when those of the incident ray and of the normal are given, with the relative index of refraction. In the original computations for this paper, the method of procedure was, to calculate first the direction-cosines of the tangent to the refracting surface in the plane of incidence. A length unity along the refracted ray was then projected on this tangent and on the normal; and these two projections were themselves projected on the axes of co-ordinates, and added. It has subsequently been found better, instead of actually calculating the direction-cosines of the tangent, to eliminate them between two sets of equations, one of which is obtained by projecting the refracted ray in the manner above described, and the other by similar projection of the incident ray.

3. A single case of special simplicity is selected for the application of the process; the case of a narrow and thin annulus of a plano-convex lens, with a parallel pencil incident at 45° on its plane face, the index being 1·5. The annulus is supposed to be graduated, the division 0° being at the point furthest from the source, and the graduations being from 0° to ± 180°. The plane containing the axis of the lens and the axis of the incident pencil passes through the divisions 0° and 180°, and is the only *plane of symmetry* of the system. For convenience of description, we shall suppose this plane to be vertical, the point 0° being the highest point of the annulus.

4. Twelve points 30° apart, starting from 0°, are taken on the annulus, as points of incidence; and the direction-cosines of the corresponding emergent rays are calculated to three or four places of decimals. The

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* See S. P. Thompson, "Experiments on Zonal Aberration," 'Archives Néerlandaises,' 1901, and Fourth Traill Taylor Lecture, printed in the 'Almanac of the Brit. Journ. of Phot.' for 1903; also Alice Everett, "Photos. of Sections of Hollow Pencils," 'Jour. Brit. Astron. Assoc.,' vol. 13, 1902, pp. 74–75.

axis of the lens is taken as axis of z, the axis of y being in and the axis of x perpendicular to the plane of symmetry. The equations of the twelve emergent rays thus found enable us to plot twelve points of the section made by a plane at any given distance z; and through these a curve can be drawn by hand.

5. These sections are, however, inclined at about 45° to the emergent rays, and are about midway between the axes of z and y. To remedy this inconvenience, the equations of the rays are transformed to new axes (of η and ζ), the axis of ζ being midway between the axes of z and y, and coinciding with the original direction of the incident beam. Sections perpendicular to this direction are found by assigning different constant values to ζ, and a "direction-curve" is drawn, which is a section of a cone whose generators are parallel to the emergent rays.

6. Harmonic reduction is applied to the direction-cosines; and harmonic expressions containing either two sines without cosines or two cosines without sines are found to give a remarkably close representation of the facts.

7. Each ray of the emergent pencil intersects two other rays. One set of intersections are in the plane of symmetry, and are the intersections of rays symmetrically placed with respect to this plane. These intersections constitute the *secondary focal line*, which is absolutely straight, and lies in the production of the straight line drawn through

downward slope of about 1 in 6. Each end of the primary line is the intersection of two ultimately coincident rays from near 79°.

After a long interval, the intersections on the secondary line begin, the first being the intersection of rays from two points ultimately coincident at 180°, and the last the intersection of rays ultimately coincident at 0°.

11. In the cross-sections, of which numerous specimens at gradually increasing distances are given in Plate 9, every intersection of two rays, or (what is the same thing) every intersection of a ray with one of the two focal lines, appears as a double point, which is generally a point of crossing of two branches, but is sometimes a cusp; and in one instance the two double points coincide in a point of contact of two branches.

For the purpose of identifying individual rays, the numbers 0, 30, 60, &c., are marked, indicating the positions, in each section, of the rays which came from the points 0°, 30°, 60°, &c., in the right hand half of the annulus. They facilitate the tracing of reversals of position.

12. It is by no means a general law for oblique refraction through annuli that the primary crossings are completed before the secondary begin. More usually there is a large region in which the two crossings overlap—a circumstance indicated by the presence of three double points in a section, the middle one of the three being in the secondary and the two outer in the primary focal line.

13. Sections of pencils from annuli of obliquely placed lenses have been calculated by Steinheil* and by Finsterwalder,† the obliquity, however, being only 0° 48′ in Steinheil's calculations, and not exceeding 6° in Finsterwalder's. In both cases, the method of computation is that devised by Seidel‡ based on spherical trigonometry; and the calculations are only for the positions 0°, ± 45°, ± 90°, ± 135°, 180°.

PART II.—*Details.*

14. *General Process for Skew Refraction.*

Let l_1, m_1, n_1 be the direction-cosines of the normal,
l_2, m_2, n_2 those of the incident ray,

then, calling the angle of incidence χ, and the angle of refraction χ, we have

$$\cos\chi = l_1 l_2 + m_1 m_2 + n_1 n_2.$$

Hence, knowing the index of refraction, we can deduce sin χ' from sin χ.

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* 'Munich, Akad. Sitz. Ber.,' vol. 19 (1889).

† 'Munich, Akad. Abhandl.,' vol. 17 (1892), p. 519.

‡ 'Munich, Akad. Sitz. Ber.,' 1866, p. 263.

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Let $\lambda\ \mu\ \nu$ denote the direction-cosines of the tangent to the refracting urface in the plane of incidence.

Then $l_2\ m_2\ n_2$ may be regarded as the projections of a unit incident ay on the axes. But this unit ray gives a projection sin χ on the angent, and a projection cos χ on the normal. Adding the projections f these on the axes, we have

$$\left.\begin{aligned} l_2 &= \lambda \sin\chi + l_1 \cos\chi \\ m_2 &= \mu \sin\chi + m_1 \cos\chi \\ n_2 &= \nu \sin\chi + n_1 \cos\chi \end{aligned}\right\} \quad \cdots\cdots\cdots\cdots \quad (1).$$

nd in the same way, by projecting a unit refracted ray, we have

$$\left.\begin{aligned} l' &= \lambda \sin\chi' + l_1 \cos\chi' \\ m' &= \mu \sin\chi' + m_1 \cos\chi' \\ n' &= \nu \sin\chi' + n_1 \cos\chi' \end{aligned}\right\} \quad \cdots\cdots\cdots\cdots \quad (2),$$

, m', n' denoting the required direction-cosines of the refracted ray.

Substituting the values of λ, μ, ν from (1), equations (2) become

$$\left.\begin{aligned} l' &= k\,(l_2 - l_1 \cos\chi) + l_1 \cos\chi' \\ m' &= k\,(m_2 - m_1 \cos\chi) + m_1 \cos\chi' \end{aligned}\right\} \quad \cdots\cdots\cdots \quad (3)$$

The Selected Case (*see* § 3).

16. The parallel rays incident at 45° on the first face, which is plane, are refracted into the lens so as to make with the normal (which is the axis of z) an angle of 28° 7½′. The sine of this angle is 0·4714, and its cosine 0·8819. The direction-cosines of the rays incident on the second or convex face of the lens are therefore—

$$l_2 = 0, \qquad m_2 = 0{\cdot}4714, \qquad n_2 = 0{\cdot}8819\,;$$

and are the same at all points.

Our calculations relate to a single narrow annulus of the convex face,* the axis of this annulus being the same as that of the lens. The radius of the annulus will be taken as the unit of length, and the centre of the annulus as the origin of co-ordinates.

The normals at all points are equally inclined to the axis of z, and we assume the sine of this inclination to be 0·1000; in other words, the radius of curvature is taken as ten times the radius of the annulus. This makes the cosine 0·9950, and the angle itself about 5° 44⅓′.

Let θ denote the angular distance of any point of the annulus from the summit (which is the farthest point from the source). Then the co-ordinates of the point are—

$$x_0 = \sin\theta, \qquad y_0 = \cos\theta, \qquad z_0 = 0,$$

and the direction-cosines of the forward-drawn normal at the point are

$$l_1 = \tfrac{1}{10}\sin\theta, \qquad m_1 = \tfrac{1}{10}\cos\theta, \qquad n_1 = 0{\cdot}9950.$$

From these we deduce, for the angle of incidence χ,

$$\cos\chi = l_1l_2 + m_1m_2 + n_1n_2 = 0{\cdot}04714\cos\theta + 0{\cdot}87749,$$

and $\sin\chi'$ is $\frac{3}{2}\sin\chi$; hence $\cos\chi'$ is known, and we have all the data for calculating the direction-cosines $l'\ m'\ n'$ of the emergent ray by equations (3). The following values are thus found:—

	0°	30°.	60°.	90°.	120°.	150°.	180°.
l'	0	−0·0286	−0·0513	−0·0622	−0·0568	−0·0342	0
m'	0·650	0·658	0·677	0·707	0·740	0·766	0·777
n'	0·760	0·753	0·734	0·704	0·670	0·641	0·630

---

* The *annulus of the lens* which corresponds to this *annulus of the convex face* may have any thickness, but description is facilitated by supposing the thickness so small as to be negligible.

nd the equations to an emergent ray are

$$\frac{x - \sin\theta}{l'} = \frac{y - \cos\theta}{m'} = \frac{z}{n'} \quad \text{......................} \quad (4).$$

When the sign of θ is reversed, the sign of l' is reversed, and there no change in m' or n'.

17. For transforming to the axes of $x\ \eta\ \zeta$ (see § 5), ζ being in the irection of the original beam incident on the first face, the required rmulæ are—

$$\eta = (y - z)\sqrt{\tfrac{1}{2}}, \qquad \zeta = (y + z)\sqrt{\tfrac{1}{2}}.$$

$$l = l', \qquad m = (m' - n')\sqrt{\tfrac{1}{2}}, \qquad n = (m' + n')\sqrt{\tfrac{1}{2}},$$

m, n denoting the direction-cosines relative to the new axes. We hus find, for the seven selected points—

	0°.	30°.	60°.	90°.	120°.	150°.	180°.
m	−0·0778	−0·0672	−0·0403	+0·00177	+0·0495	+0·0880	+0·1040
n	0·997	0·998	0·998	0·998	0·997	0·995	0·995
$\eta_0 = \zeta_0$ ..	0·7071	0·6124	0·3536	0	−0·3536	−0·6124	−0·7071

It commences (at distance 7·72) with contact of the two branches, their common tangent being also a tangent to the primary focal line. The lower branch then passes through the upper, giving three areas separated by two points of self-cutting. These two points then travel outwards, causing the two loops beyond them to become smaller, till, at distance 8·51, the loops have vanished and are replaced by cusps, which are at the two ends of the primary focal line.

The third row consists of sections intermediate between the two focal lines, the cusps having changed to rounded off angles, and one of the sections (at distance 11) being approximately an equilateral triangle with corners rounded off. The last of them, at distance 13, has a fairly sharp angle at the top.

The fourth row consists of seven sections through the secondary line and one at a greater distance. The first has a cusp at the top, changing into a small loop, and then we have figures of 8, with the upper loop enlarging and the lower diminishing, till at distance 18·51 the lower loop has been replaced by a cusp. There is thus a cusp at each end of the secondary (as well as of the primary) focal line. When the figures are produced experimentally by receiving the pencil on a card, it is possible to hold the card in a very oblique position in which the two sides of the figure 8 unite, and give a single line resembling the image of a slit. This image is the secondary focal line.

When the secondary line is passed, the area of the section increases rapidly. The form is at first that of an oval sharpened at the bottom, as shown in the last figure of the Plate; but, as the distance increases, the two ends become nearly alike; and at still greater distances, the lower half becomes broader and blunter than the upper, as exemplified in the section at infinite distance which stands first in Plate 10.

19. In the figures of Plate 9, the zero point $\xi = 0$, $\eta = 0$, through which an undeviated ray from the origin would pass, is indicated by a large dot, and it always lies below the centre of figure—a consequence of the preponderating action of the lower portion of the annulus. At distances 6 and 9·9 the zero point coincides with the 180° point and the 0° point respectively, and at intermediate distances it *lies outside the curve.* This explains the observed fact that, when two concentric annuli are employed, differing greatly in size, the curve due to the inner often lies outside that due to the outer. In dealing with the solid pencil given by a very obliquely placed lens, it is clearly not permissible to identify the bounding surface of the pencil with the surface constituted by rays from the outermost zone.

20. The direction-cosines l', m', n', are approximately simple-harmonic functions of the angular co-ordinate θ of the point of emergence. The amplitude of the chief term (of period 360°) is some fifteen or twenty *times as great as that* of the next term (of period 180°); and when

oth these terms are included, the largest departures from agreement ith the twelve data amount to about $\frac{1}{300}$ of the chief amplitude for , and $\frac{1}{70}$ for m' and n'. The harmonic expressions to which these marks apply are

$$l' = -0{\cdot}0624 \sin\theta + 0{\cdot}0032 \sin 2\theta,$$

$$m' = 0{\cdot}7103 - 0{\cdot}0628 \cos\theta + 0{\cdot}0033 \cos 2\theta,$$

$$n' = 0{\cdot}6997 + 0{\cdot}0645 \cos\theta - 0{\cdot}0047 \cos 2\theta,$$

om which are deduced

$$m = 0{\cdot}0075 - 0{\cdot}0900 \cos\theta + 0{\cdot}0057 \cos 2\theta,$$

$$n = 0{\cdot}9970 + 0{\cdot}0013 \cos\theta - 0{\cdot}0010 \cos 2\theta.$$

The maximum error for m is about $\frac{1}{50}$ of the amplitude.

As regards n, the largest departure from agreement is about $\frac{1}{2000}$ f the large constant term 0·9970, and therefore does not materially ffect the values of x and η calculated from

$$x = \sin\theta + \frac{l}{n}(\zeta - \zeta_0), \qquad \eta = \eta_0 + \frac{m}{n}(\zeta - \zeta_0),$$

accordance with equations (5)

$x = 0$ in equations (4) or (5) of §§ 16, 17, using the values of l', m', n', or of l, m, n, there given for $\theta = 30°, 60°, \ldots 150°$. The two which correspond to the points 0° and 180° can in like manner be found by applying equations (3) to values of θ indefinitely near to 0 and π respectively.

Near 0° we thus obtain, for small θ,

$$\frac{x-\theta}{-0{\cdot}056620\theta} = \frac{y-1}{0{\cdot}6505} = \frac{z}{0{\cdot}7595};$$

and near 180°, for $\pi - \phi$ with ϕ small,

$$\frac{x-\phi}{-0{\cdot}06966\phi} = \frac{y+1}{0{\cdot}7768} = \frac{z}{0{\cdot}6297},$$

which, for $x = 0$, give

$$\begin{array}{l|ll|ll} \theta = 0°, & y = 12{\cdot}50, & z = 13{\cdot}43 & \eta = -0{\cdot}66, & \zeta = 18{\cdot}34.^{*} \\ \theta = 180°, & \quad 10{\cdot}15, & \quad 9{\cdot}04 & \quad 0{\cdot}785, & \quad 13{\cdot}57. \end{array}$$

The two points thus determined are the ends of the secondary focal line, and the other five points when plotted are found to be sensibly in the straight line joining these two. The tangent of the inclination of the line to the axis of ζ, as computed from the co-ordinates $\eta\,\zeta$ of its ends, is $\dfrac{1{\cdot}445}{-4{\cdot}77} = -{\cdot}303$.

Sections of the pencil made through the secondary line, are figures of 8 (see 4th row of Plate 9); the crossing point of the 8 being the point in which the line meets the section. At the ends of the line, one loop of the 8 vanishes and is replaced by a cusp. The cusps are shown separately with tenfold magnification, in Plate 10.

23. A simple application of descriptive geometry suffices to show that, in every case of refraction of a homocentric pencil at a spherical surface, all the refracted rays pass through a straight line, namely, the straight line which joins the point-source S to the centre of curvature C of the refracting surface. For, if P be the point of incidence on this surface, PC is the normal, and SPC the plane of incidence. The refracted ray lies in this plane, and therefore meets the line SC. In the case with which we are dealing, S is at infinity, and SC is parallel to the rays within the lens.

24. Again, the plane SCP cuts the annulus in a second point P′, and the plane of incidence SCP′ is identical with SCP. The refracted rays at P and P′ lie in this plane, and their intersection is a point in the primary focal line.

---

* It is interesting to compare the distance 18·34 of the further end of the secondary line with the focal length of the annulus, which is 19·78.

25. Applying these principles, we have

$$\text{Deviation at first refraction} = 45^\circ - 28^\circ\ 7\tfrac{1}{2}' = 16^\circ\ 52\tfrac{1}{2}',$$

which is accordingly the downward slope of the rays in the glass. The tangent of 16° $52\frac{1}{2}'$ is 0·303, which agrees with the value above deduced from the co-ordinates of the ends of the secondary line.

Again, the distance OC of the centre of curvature from the origin is $\sqrt{99} = 9{\cdot}950$; and the condition that C is collinear with the ends of the secondary line is the vanishing of the determinant.

$$\begin{vmatrix} 12{\cdot}50, & 10{\cdot}15, & 0 \\ 13{\cdot}43, & 9{\cdot}04, & -9{\cdot}95 \\ 1, & 1, & 1. \end{vmatrix}$$

Expanding, we find that the positive terms amount to 237·4, and the negative to 237·3, a sufficiently close agreement.

26. Let A in the annexed figures be the point where a line through C parallel to the rays in the lens meets the plane of the annulus. If we draw any straight line through A, cutting the annulus in two points P, P′, the rays at these two points are in the same plane of incidence

θ	0°	30°	60°	90°	120°	150°
θ'	180°	136° 10′	99° 35′	68° 40′	43° 4′	20° 44′

besides $\theta = \theta' = 79° \; 10'$.

27. To trace the primary focal line, I have calculated, by formulæ (3), the equations of the rays (in terms of $x\,y\,z$) at these thirteen points, and plotted on a large scale the sections of the rays by planes of constant z, the smallest value of z being 5·304, which corresponds to the intersection of the rays from 0° and 180°, the largest 6·05, which corresponds to 79° 10′, and the others being 5·4, 5·5, 5·6, 5·7, 5·8, 5·9, 6·0. Some of these sections* are reproduced in Plate 10. The co-ordinates $x\,y$ of the points of self-cutting of the curves (drawn carefully by hand through the plotted points) were adopted as the co-ordinates of points of the primary line; and the results are exhibited in Plate 10 in the shape of three curves which are the projections of the primary line on the co-ordinate planes of $x\,\eta\,\zeta$. These projections are on the same scale as the curves in Plate 9. The scale of the intersecting curves is five times as large. The projection on the plane of $\eta\,\zeta$ (the plane of symmetry) is very nearly a straight line. The other two projections show that the form of the primary line is approximately parabolic. The length of the chord joining its ends is 0·952, and the distance of this chord from the vertex, 0·911. The tangent of the slope of the approximate plane of the curve is about $-\frac{1}{6}$; and the ends of the curve (which are its lowest points) are just above the plane of $x\,\zeta$.

28. A general view of the system as projected on the plane of symmetry is given (on one-fourth the scale of Plate 9) at the foot of Plate 10. The highest and lowest points of the annulus are marked 0 and 180, and the rays incident at these two points are traced as far as their meeting with the secondary focal line. C is the centre of curvature of the convex face of the lens, the radius of curvature being ten times the radius of the annulus. CA, meeting the plane of the annulus in A, is parallel to the rays within the lens, and its production coincides with the secondary focal line.

Cusps in the Sections.

29. Every cross-section through an end of a focal line contains a cusp, which is the transition from a small loop to a rounded-off angle in sections taken near it. Three such sections, either of constant z or of constant ζ can be taken; namely, one section through each end of the secondary, and a single section through both ends of the primary.

30. The values of $dx/d\theta$ and $dy/d\theta$ for constant z, and of $dx/d\theta$ and $d\eta/d\theta$ for constant ζ, vanish at a cusp, and are very small in the region

---

* The markings 21, 43, 69, 100, 136, 79, against the curves are abbreviations for *20° 44′, 43° 4′, 68° 40′, 99° 35′, 136° 10′, 79° 10′.*

round it. This property is of great assistance in locating the ends of he focal lines. Physically interpreted, it indicates close aggregation f rays, and consequent increase of luminous intensity, at the ends f the two focal lines.

31. To find at what distant z from the plane of the annulus the alues of x and y for the ray at 0° are stationary for changes of θ.

We have in general

$$x = \sin\theta + \frac{l'}{n'}z; \qquad y = \cos\theta + \frac{m'}{n'}z.$$

Hence, for constant z,

$$\frac{dx}{d\theta} = \cos\theta + \frac{z}{n'^2}\left(n'\frac{dl'}{d\theta} - l'\frac{dn'}{d\theta}\right),$$

$$\frac{dy}{d\theta} = -\sin\theta + \frac{z}{n'^2}\left(n'\frac{dm'}{d\theta} - m'\frac{dn'}{d\theta}\right).$$

Differentiating the harmonic expressions (§ 20) for l' m' n', we see that the expressions for $dm'/d\theta$ and $dn'/d\theta$ contain only sines, and consequently vanish at 0°. Hence $dy/d\theta$ vanishes identically at 0°, and $dx/d\theta$ reduces to $\cos\theta + \frac{z}{n'}\frac{dl'}{d\theta}$, which is to vanish.

The form of these cusps, for a section of constant z, is exhibited near the centre of Plate 10, on half the scale of the two cusps of the secondary (which are for sections of constant ζ).

33. The following examples illustrate the use of the harmonic formulæ:—

(*a*) To find the points of the annulus at which the deviation is exactly perpendicular to the plane of symmetry—

We must put $m = 0$, giving

$$900 \cos \theta - 57 \cos 2\theta = 75, \text{ whence } \theta = \pm 88° \, 51'.$$

(*b*) To find the points at which the horizontal deviation is greatest, we must put $dl/d\theta = 0$, giving

$$\frac{\cos 2\theta}{\cos \theta} = \frac{624}{64} = 9\tfrac{3}{4}, \qquad \theta = \pm 95° \, 46'.$$

(*c*) For the total deviation to be a maximum or minimum, its cosine n must be a minimum or maximum. We thus obtain—

$$\frac{dn}{d\theta} = -0{\cdot}0013 \sin \theta + 0{\cdot}0020 \sin 2\theta,$$

$$= -0{\cdot}0001 \sin \theta \, (13 - 40 \cos \theta) = 0.$$

The factor $\sin \theta$ vanishes at 0° and 180°, at which there are maxima. The factor $13 - 40 \cos \theta$ vanishes for $\theta = \pm 71°$, and gives the points of minimum deviation.

The points on the "direction curve" which correspond to these three determinations are indicated by the words "horizontal," "maximum," "minimum," on the right hand of the curve.

(*d*) To find at what distance z the radius of curvature of a cross section becomes infinite at one of the two points 0° and 180° in the section.

From equations (4) we deduce

$$\frac{dx}{d\theta} = \cos \theta + z \frac{d}{d\theta} \frac{l'}{n'},$$

$$\frac{dy}{d\theta} = -\sin \theta + z \frac{d}{d\theta} \frac{m'}{n'}.$$

At 0° and 180°, $dx/d\theta$ is finite, but $dy/d\theta$ vanishes (since $dm'/d\theta$ and $dn'/d\theta$ contain only sines). Hence the tangent is horizontal. The condition of an infinite radius of curvature is $d^2y/d\theta^2 = 0$; or

$$-\cos \theta + z \frac{d^2}{d\theta^2} \frac{m'}{n'} = 0.$$

For the 0° ray this is

$$\frac{1}{z} = \frac{d^2}{d\theta^2}\frac{m'}{n'}.$$

and for the 180° ray

$$\frac{1}{z} = -\frac{d^2}{d\theta^2}\frac{m'}{n'}.$$

We have

$$\frac{d}{d\theta}\frac{m'}{n'} = \frac{1}{n'}\frac{dm'}{d\theta} - \frac{m'}{n'^2}\frac{dn'}{d\theta},$$

$$\frac{d^2}{d\theta^2}\frac{m'}{n'} = \frac{1}{n'}\frac{d^2m'}{d\theta^2} - \frac{m'}{n'^2}\frac{d^2n'}{d\theta^2} + \&c.,$$

the &c. consisting of terms which vanish at 0° and 180°. We thus obtain at 0°, $z = 8{\cdot}554$, and at 180°, $z = 3{\cdot}530$.

As the curves are symmetrical about the axis of y, the first differential coefficient of y that does not vanish must be of even order. It is accordingly of the fourth order at the two points thus determined; and the curve is sensibly straight for a considerable distance.

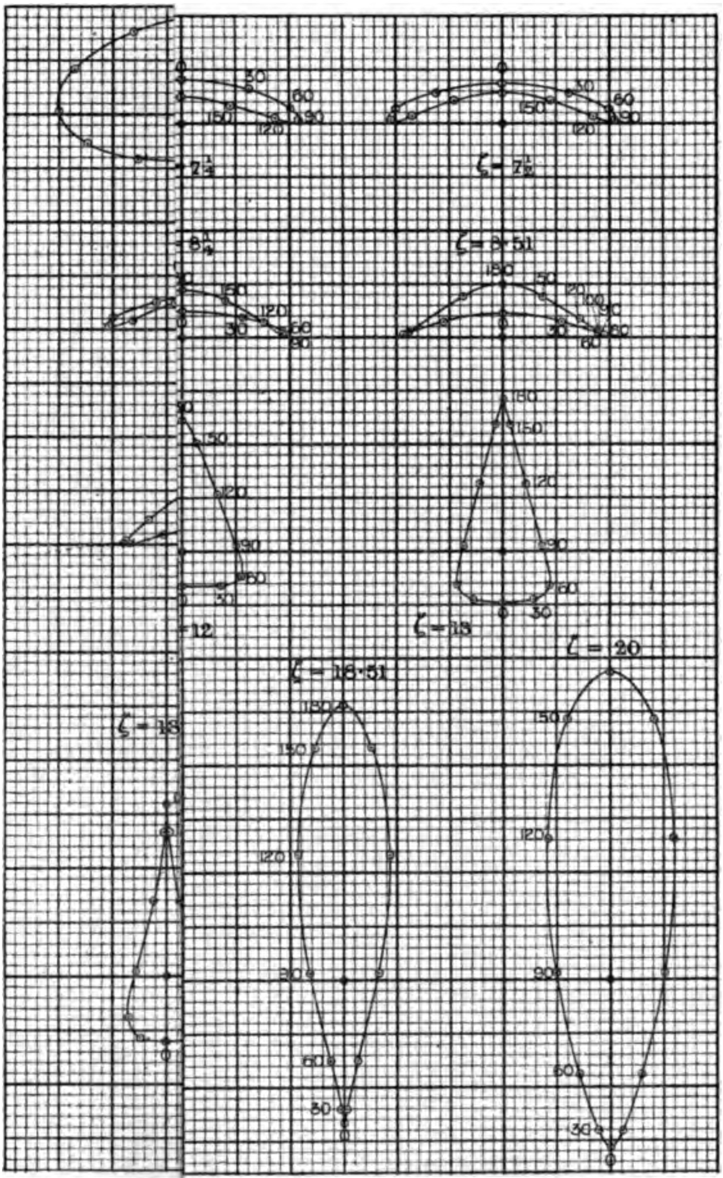
If, instead of sections of constant z, we take sections of constant ζ through these two points, the conclusions remain true; for, by Meunier's theorem, the radii of curvature in the two sections are in a finite ratio. From the above values of z, together with the equations of the rays, we can deduce the values of y, and then transform to $\eta\ \zeta$. We thus find—

for $\theta = 0°$, $\zeta = 11{\cdot}94$, for $\theta = 180°$, $\zeta = 4{\cdot}86$,

results which are illustrated by the sections for

$\zeta = 12$, $\zeta = 4$, and $\zeta = 6$, in Plate 9.

Everett. *Roy. Soc. Proc., vol.* 71, *Plate* 9.



For the 0° ray this is

$$\frac{1}{z} = \frac{d^2}{d\theta^2}\frac{m'}{n'} .$$

and for the 180° ray

$$\frac{1}{z} = -\frac{d^2}{d\theta^2}\frac{m'}{n'} .$$

We have

$$\frac{d}{d\theta}\frac{m'}{n'} = \frac{1}{n'}\frac{dm'}{d\theta} - \frac{m'}{n'^2}\frac{dn'}{d\theta} ,$$

$$\frac{d^2}{d\theta^2}\frac{m'}{n'} = \frac{1}{n'}\frac{d^2m'}{d\theta^2} - \frac{m'}{n'^2}\frac{d^2n'}{d\theta^2} + \&c.,$$

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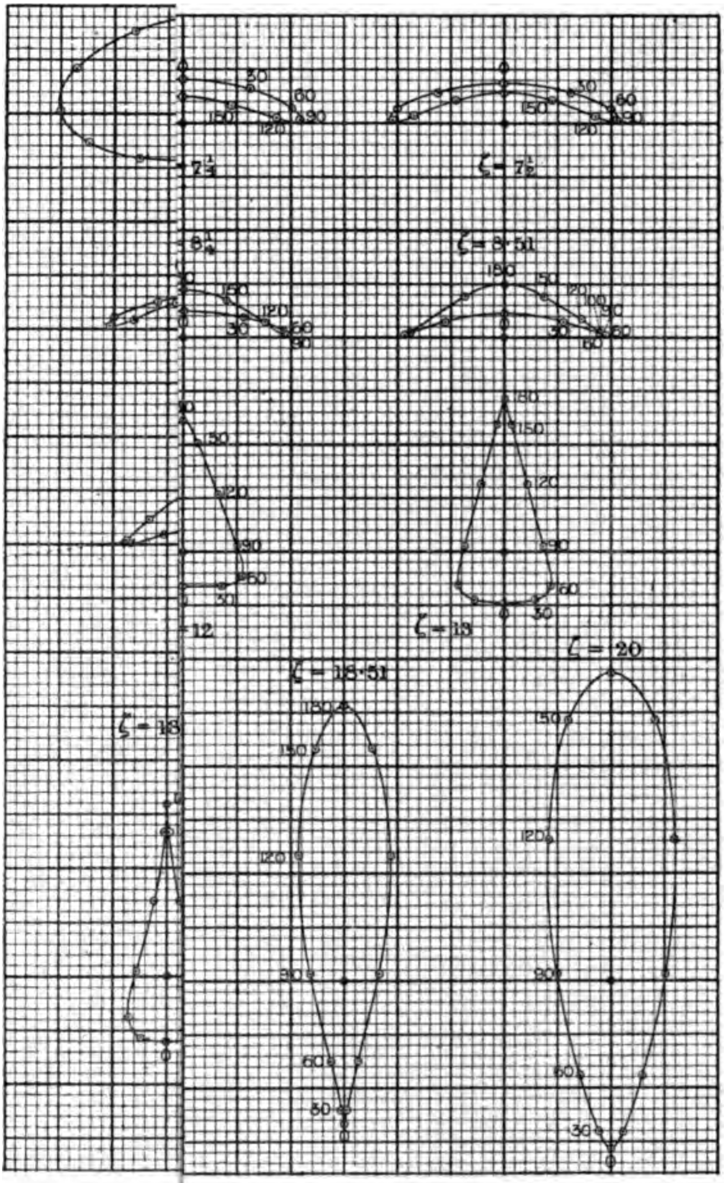

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REPORTS TO THE EVOLUTION COMMITTEE OF THE ROYAL SOCIETY.

REPORT I.—EXPERIMENTS UNDERTAKEN BY W. BATESON, F.R.S., AND MISS E. R. SAUNDERS.

(Presented to the Committee, Tuesday, December 17, 1901.)

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EXPERIMENTAL STUDIES IN THE PHYSIOLOGY OF HEREDITY.

By W. BATESON, M.A., F.R.S., and Miss E. R. SAUNDERS, Lecturer, Newnham College, Cambridge.

INTRODUCTION.

The experiments described in this Report were for the most part begun in 1897, and are still in progress. A part of the cost of the work has been defrayed from the Government Grant Fund, the sums thus received having been in part allotted to us by the Evolution Committee, and in part directly by the Grant Committee. The use of the garden in which the botanical experiments were carried out was kindly permitted during 1901, rent free, by the Botanic Garden Syndicate.

Following recognition of the fact that variations of animals and plants may be distinct phenomena, according as they are continuous or discontinuous, it seemed desirable to ascertain in as many cases as possible which variations belong to each of these categories. To answer this question specifically with regard to a diversity of forms, taking each character separately, and testing it by direct experiment in cross-breeding, was a primary object of our work. Such determinations must, in our judgment, constitute a first step in attempting to proceed with the problem of the nature and origin of Species.

Moreover, from what had been hitherto ascertained regarding the phenomena of heredity, the inference could scarcely be avoided that no universal law obtains, but that by studying various specific cases distinct specific laws may be detected.

Another object we have kept in view, is to test the possibility that in-breeding may have a specific effect in modifying the power of transmitting parental character to offspring. There are some indications,* lerived from various sources, which point to such a possibility, and end to show that an in-bred individual of a given breed A, when rossed with an individual of a different breed B, will be more likely o "transmit" the A characters to the offspring—in ordinary terms more ' prepotent"—than another individual of the same breed A which was ot in-bred, though equally pure or "thoroughbred." It did not ppear that this possibility had been properly investigated by scientific methods, though otherwise expressed it is not unfamiliar to practical reeders. For example it is commonly believed that, *cæteris paribus*, a thoroughbred animal is more likely to stamp its own character on its ffspring than an animal which is not a thoroughbred.

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* The most remarkable illustration of this phenomenon is that which is alleged

Thoroughbred animals are not rarely in-bred, and the possibility that the prepotency may be due to the in-breeding rather than the pure-breeding should be borne in mind. It is needless to emphasise the great practical and scientific consequences of such a proposition if it could be established.

Five years ago nothing could have seemed simpler, practical ifficulties apart, than to investigate this question and to put it to an experimental test.* Meanwhile, however, as will presently appear, the whole problem of heredity has undergone a complete revolution, and the old idea of prepotency, if it is to be retained at all, must be otherwise conceived. Evidence of greater or less prepotency obtained by methods formerly supposed satisfactory is now capable of different interpretations, and it is clear that to obtain a definite result on this point, a new set of precautions must be used. Consequently though some of the evidence to be adduced bears on the question of differential prepotency, we can offer as yet no plain answer to the original question, and some time must elapse before the matter can be definitely determined.

We expected, then, in chosen cases to determine the degree to which certain characters are discontinuous, whether they are capable of blending with their contraries or not, and to what degree their distinctness is maintained on crossing. In suitable cases we hoped further to determine specific laws of heredity, and to ascertain whether these laws can be affected by in-breeding the parental forms. It has always appeared to us that it can only be by *exhaustive study of special cases* that success can be attained in this direction, and that no results are as yet to be expected from partial experiment with a multitude of forms.

Upon these original objects and on several collateral subjects our experiments have given information. They are still, however, in the initial stage, and we hope to continue them in future years. We greatly regret that from want of space and appliances, we have been obliged to reduce the scope of our experiments to the barest limits. Investigations of this kind to be properly pursued—with animals at least—require far greater resources than we are able to command.

For many reasons we would rather have deferred publication until the work was further advanced, but we feel that with the re-discovery and confirmation of the principle which will henceforth be known as Mendel's Law, the study of heredity and the cognate problems of evolution must enter on a new phase.

At the present time the question how far Mendel's Law with its numerous corollaries is valid, to what cases and to what extent it is of general applicability, supersedes all others in significance. Consequently, our experience, however imperfect, since it bears directly on this question, has a value at this juncture, and we think it may be of use to the many investigators who will doubtless now turn their attention to the experimental study of heredity.

In the history of evolution it must ever be an astonishing fact that Mendel's discovery has so long remained unknown. The obscurity of the Brünn journal is a circumstance quite insufficient to explain the neglect of such a work.* No careful reader could doubt for a moment that he was in presence of true facts of exceptional significance. The reason may be that at the time of Mendel's publication, the announcement of the principle of Natural Selection had almost completely distracted the minds of naturalists from the *practical* study of evolution. The labours of the hybridists were believed to have led to confusion and inconsistency, and no one heeded them any more.

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* "Abh. naturf. Ver. Brünn," 1865, vol. 4, p. 1. The author of this remarkable paper, Gregor Johann Mendel, according to a communication made to Correns by Dr. von Schanz, was born in 1822 at Heinzendorf-bei-Odrau in Austrian Silesia. His parents were well-to-do peasants. In 1843 he entered the Königinkloster, an Augustinian house in Altbrünn and was ordained priest in 1847. From 1851–1853 he studied physics and natural sciences at Vienna. On retiring to his cloister he became a teacher in the Realschule of Brünn and afterwards Abbot. He died in

It is, perhaps, even more surprising that other investigators failed to discover the same law. Naudin's conclusions came very near it.

The literature of breeding teems with facts now palpably Mendelian. Gärtner, Godron, Laxton, even Darwin himself, must have been many times on the brink of the discovery.

Looking now at such experiments as those of Rimpau with wheat, &c., of Laxton with *Pisum*, Godron with *Datura*, of Darwin with Antirrhinum and sweet peas, we can hardly understand how the conclusion was missed. In Darwin's case no doubt the theory of Pangenesis contributed to difficulty, but some part of the obscurity must have been due to the habit of regarding various species, breeds, varieties, and casual fluctuations as all comparable expressions of one phenomenon, similar in kind; and to insufficient recognition of the possibility that variation may be, in its essence, specific. We may, perhaps, attribute to this preconception the extraordinary complexity with which his experiments on poultry and pigeons were arranged.

as our own views and modes of expression do not agree wholly with those of other writers on the subject.

It may be premised that the first publication of the re-discovery was made in 1900 by de Vries,* and almost simultaneously by Correns† and Tschermak.‡ There can be no doubt that the appearance of this group of papers constitutes at length a definite advance both in the general study of the physiology of reproduction and in the particular problem of the nature of Species.

In 1865 Gregor Mendel published his discovery. His experiments are described in considerable detail, and the resulting law is stated with emphasis and precision. He at once perceived and lucidly enunciated what he regarded as the essential truth underlying the observed facts. A reader of this paper can hardly fail to recognise its masterly quality. By some strange chance it was ignored, and excepting a casual reference in Focke's 'Pflanzen-Mischlinge' (p. 110) there appears to be no allusion in literature to this remarkable performance. It is a fortunate circumstance that we need feel no hesitation in now accepting Mendel's account; for even if the original paper were such as to admit of doubt, the re-discovery comes to us with a large body of fresh evidence, the simultaneous work of three independent observers, confirming Mendel on the main points.

Mendel states that, like other investigators, he had been struck by the regularity with which offspring of certain hybrids reproduce the pure ancestral forms. But owing, as he supposes, to the complex nature of the cases studied and to want of accurate statistics, the precise facts had never been ascertained. Accordingly he set himself to work out some case from which every confusing element should as far as possible be excluded. After several trials he chose the varieties of *Pisum sativum* as best suited to his purpose. Besides other advantages, this species is well known to have the exceptional merit of being habitually self-fertilised, in N. Europe at least. From the many varieties of peas he first chose pairs of varieties, for crossing, in such a way that the members of each pair differed from each other in respect of one definite character. Of such pairs of characters he chose seven, namely: 1. Shape of seed, whether rounded *or* irregularly angular and deeply wrinkled. 2. Colour of cotyledons [" *endosperm*"], whether some shade of yellow *or* green. 3. Colour of seed-skin, whether a brownish shade *or* white (in correlation with white flowers). 4. Shape of ripe pod, whether simply inflated *or* deeply constricted between the seeds. 5. Colour of unripe pod, whether a shade of green *or* bright yellow. 6. Position of flowers, whether distributed along the stem *or* crowded near the top in a false umbel. 7. Length of stem, whether about 6—7 feet *or* about $\frac{3}{4}$—$1\frac{1}{2}$ feet. Between these various pairs of varieties crosses were then made, the female parent being emasculated.

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* De Vries, 'Comptes Rendus,' March 26, 1900, and 'Ber. deut. Bot. Ges.,' xviii, 1900, p. 83; *ibid.*, p. 435; 'Rev. gén. Bot.,' 1900, p. 257.

† Correns, 'Ber. deut. Bot. Ges.,' xviii, 1900, p. 158; 'Bot. Ztg.,' 1900, p. 232.

‡ Tschermak, 'Ztschr. f. d. landw. Versuchswesen in Oesterr.,' 1900, 3, p. 465.

As to the first two characters the result of the experiment is seen as soon as the cross-bred seeds are ripe; but to study the other five characters it is of course necessary to plant the seeds and grow the plants to maturity. On such examination it was found that in the case of each pair of characters one only was manifested in every cross-bred individual, to the total, or almost total, exclusion of the opposite character. The character which thus prevails is, in Mendel's terminology, the *dominant* (D), the character which is suppressed, being *recessive* (R). In the above enumeration of characters the dominant is placed first in each pair. Reciprocal crosses gave identical results. Briefly, then, D × R or R × D gave offspring which in appearance are all practically D.

The next generation is obtained by allowing the cross-bred plants DR to fertilise themselves. The result of such self-fertilisation is, according to Mendel, that the next generation instead of being uniform like their parents, breaks up into the two original forms. This takes place in such a way that there are on the average *three dominants to one recessive.*

The recessives are thenceforth not only apparently but *actually* pure the results, *as average results*, are clear. The purity of the pure dominants and pure recessives resulting from the self-fertilisation of the cross-breds was tested in the case of characters 1 and 2 to the sixth generation, for characters 3 and 7 to the fifth generation, and for characters 4, 5, and 6 to the fourth generation.

Mendel interprets his facts as follows:—They point to the conclusion that in the cross-bred each of its pollen-grains and each of its egg-cells is *either pure dominant or pure recessive*,* *and that on the average there are equal numbers of each kind for each sex.* If the assortment of pollen-grains and egg-cells is then supposed to take place at random, the most probable percentage result is 25D ♀ × D ♂, 25D ♀ × R ♂, 25R ♀ × D ♂, and 25R ♀ × R ♂. As D × R does not differ from R × D, we have therefore 25D : 50DR : 25R. But as cross-breds resemble dominants in appearance, the *apparent* result is 3D : 1R at each successive self-fertilisation of cross-breds.

Mendel next proceeded to cross pairs of varieties differing in respect of *two* characters; for example, a variety having seeds round and yellow with a variety having seeds angular and green; and also pairs of varieties differing in *three* characters; for example, a variety characterised by seeds round, yellow, and in grey-brown skins with a variety having seeds angular, green, and in white skins. The numerical results of these experiments are set out at length, and are too long to repeat. Briefly the result in all cases was that the dominant characters alone appeared in the first cross. When these were self-fertilised, in the case of *two* differentiating characters, *four* different kinds of seeds resulted, namely, round yellow, angular yellow, round green, and angular green. In the case of *three* differentiating characters these were similarly combined in the *eight* possible combinations. In both sets of experiments the numbers of individuals and their constitution, as tested by the seeds they were capable of producing on self-fertilisation, were consistent with the hypothesis arrived at in the case of varieties differing in respect of one character, namely, that each male and female cell of the cross-bred is pure in respect of one character of each pair of characters, and is capable of transmitting this character to the exclusion of the opposite character; that the reproductive cells are, in the cross-breds, of as many kinds as there are possible combinations of pure characters (taken two or three together, as the case may be); and, finally, that each kind is represented in the cross-breds on the average in equal numbers.

Mendel next tested the truth of the hypothesis that both male and female cells were similarly differentiated, by crossing the first crosses with pure D and pure R forms respectively, finding, as he expected, that DR × D gives the ratio 1DD : 1DR, and that DR × R gives on the average the ratio 1DR : 1RR. Both sets of germ-cells are therefore similarly differentiated.*

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* In what follows such forms are spoken of as "extracted" dominants, or "extracted" recessives.

It must be understood that this condensed statement does no justice to the lucidity and completeness of Mendel's account of his facts, and of the reasoning based on them. With regard to the numbers, it may be said that though there is a good deal of irregularity, yet taken together they plainly bear out the law as a statement of average results.

De Vries† working with pairs of varieties belonging to a diversity of genera and species, found that in a large number of cases one of the varietal characters was definitely dominant, prevailing in the first crosses to the exclusion of the recessive character. In several of these cases the offspring of the cross-breds fertilised *inter se* were mixed dominants and recessives in proportions fairly agreeing with Mendel's law. In the case of two colour varieties of *Papaver somniferum*, the constitution of the resulting dominants was investigated, and shown to be also according to the law. In certain cases also the purity of the recessives was tested and found to be complete.

De Vries' first announcement was followed almost immediately by the appearance of a paper by Correns,‡ giving an account of some years' work with peas, repeating Mendel's experiments and confirming them as regards the colour of the seeds.

with Mendel's hypothesis. He believes also that there is evidence that yellow is more decidedly dominant over green than the rounded character is over the wrinkled, and on this point further experiments are required.

Subsequently a paper by Correns* has appeared describing his experiments with a glabrous and a hoary form of garden stock (*Matthiola incana*), and giving results as to these varieties, tested by self-fertilisation, and also by recrossing the cross-breds with the parental forms. In this, as in each of the other papers, there are many points which call for separate notice and discussion, but the facts taken together are in fairly close agreement with the expectation given by Mendel's law, though the discrepancies are decidedly greater in this case than in the others. The case happens to be one which had also formed a subject of our experiments, and the various questions raised by the facts will be discussed in connection with the statement of our results.

Lastly, Professor Correns has also published† an elaborate and important memoir on the results obtained in crossing varieties of maize. Some of these results are of a complex character, but the essential fact of the truth of Mendel's law in its application to many of these cases was fully established. From the striking differences between these varieties in several characters, notably in the constitution of the endosperm, whether round starchy (dominant) or wrinkled and sugar-containing (recessive), this case is a most attractive subject for such experiments.

In the characters of the endosperm in maize de Vries‡ also demonstrated the truth of Mendel's law. As regards the colour varieties, Correns showed many complications to exist owing to the fact that the general appearance of the seed depends partly on maternal and partly on embryonic elements; and in some of the characteristics there are other confusing factors owing to fluctuations in the intensity of the dominance.

The above is a brief sketch of a vast mass of observations all tending to the same conclusion, and the truth of the law enunciated by Mendel is now established for a large number of cases of most dissimilar characters, beyond question.

Let us now shortly distinguish what is essential in the new discovery from what is not. The fact that in the cross-bred one character, in appearance, dominated to the exclusion of the other is not of the essence of Mendel's discovery. Tschermak, for instance, saw some exceptions to this rule in *Pisum*. Among the *Matthiola* crosses here described, and among those originally made by Trevor Clarke,* the hoary form *seems* to be not exclusively dominant. Correns saw considerable fluctuations in the dominance of the colours of maize. It is practically certain some of the poultry cases about to be described exhibit the same phenomenon. In fact, generally speaking, there are good reasons for thinking that in numerous instances purity of gametes may occur without either character exhibiting dominance.

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* 'Bot. Cblt.,' 1900, 43, p. 97.

† 'Biblioth. Bot.,' 1901, Heft 53.

‡ 'Rev. gén. Bot.,' 12, 1900, p. 270.

There is also some reason for supposing that alterations in dominance may be affected by changes in conditions (see p. 136), and possibly even by differences in the state of the parents, though the only considerable body of evidence relating to these points, obtained by Vernon† in Echinid crossing, still needs repetition and confirmation.

The essential part of the discovery is the evidence that *the germ-cells or gametes produced by cross-bred organisms may in respect of given characters be of the pure parental types and consequently incapable of transmitting the opposite character:* that when such pure similar gametes of opposite sexes are united together in fertilisation, the individuals so formed and their posterity are free from all taint of the cross; *that there may be, in short, perfect or almost perfect discontinuity between these germs in respect of one of each pair of opposite characters.*

Doubt might naturally be felt as to the acceptance of a proposition so far-reaching, but it is impossible to see any other possible interpretation *napi* are highly complex. In the light of present knowledge we desire to repeat these experiments, and we hope to clear up some of the difficulties before publishing. To those who have leisure for such work, we suggest these species as offering unusually good opportunities for investigating problems of heredity, and we should be glad to communicate with any one who cares to undertake such work.

PART I.—EXPERIMENTS WITH PLANTS, CARRIED OUT BY E. R. SAUNDERS.

Choice of Subjects for Experiment.

In "The Variation of Animals and Plants under Domestication,"* Darwin mentions among other instances of plants in which the absence of blending of the parental characters in the offspring is well-marked, the case of certain hoary and glabrous varieties of garden stocks; when these varieties were intercrossed the foliage of the offspring resembled one or other of the two parents, and intermediates did not occur. The evidence upon which this statement rests is furnished by the experiments of Trevor Clarke, a brief reference to which is found in the 'Gardener's Chronicle,'† and a fuller statement in the Report of the Proceedings of the International Horticultural Exhibition and Botanical Congress, held in London in 1866.‡

In these experiments the "little annual glabrous-leaved stock of the nurserymen" was crossed with pollen from a large hoary biennial form (*Cocardeau* of the French); the result was that one half of the seedlings were glabrous or "Wall-flower leaved," and the other half rough like the male parent—none were intermediate in leaf character.

The glabrous seedlings were, however, no longer dwarf but "of tall and strong habit." The author further states that succeeding generations raised from the hoary cross-breds produced a percentage of glabrous plants, but that the converse was not observed and probably did not occur. Moreover these experiments were verified by repeated trials, which were followed in every case with the same result. Similar results were obtained from intercrossing races which differed in seed colour, as *e.g.*, a purple flowered Queen stock with black seeds, and a red flowered stock with brown seeds. These statements suggested that the stock would afford excellent material for statistical experiments upon cross-breeding. The choice of this genus has, moreover, the following advantages: the plants are hardy and flower freely; the flowers are fairly easy to handle; self-fertilisation occurs in the course of nature, so that a succession of generations, thus raised, can be obtained without other treatment than that of covering the flowers; the seeds

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* Ed. 2, vol. 2, p. 71.

† 1866, p. 588.

‡ p. 143.

ment, viz., *Atropa Belladonna* ty
at the time were available in t
Datura Stramonium and *D. Tatul*
fruited forms have appeared to
on hybridisation to be favourable

These experiments were begun
the papers† in which he draws a
Mendel, and enumerates various ca
law holds, including among them
glabra, and *Datura Tatula* and *Str*
Correns‡ published an account o
showing to what extent Mendel's l
stocks which he employed. In add
it seems desirable at this point to gi
results obtained in our experiments,
they have revealed more complex is
examined by Correns.

Throughout these experiments
emasculated (when hermaphrodite),
of the anthers and the unfolding c
usually taken from covered flower
unopened; in the case of some hoa
or at a distance from those under
was considered unnecessary. In bo
the plant and the disposition of the
buds expanding simultaneously on a
an arrangement which minimises

have been possible without seriously reducing the number of experiments: except in special cases therefore, it was not attempted.

Preliminary Statement of Results.—It may be stated at once that the phenomena in Lychnis, Atropa, and Datura follow Mendel's law with considerable accuracy, and no exceptions that do not appear to be merely fortuitous were discovered.

In the case of Matthiola, the phenomena are much more complex. There are simple cases which follow Mendelian principles, but others of various kinds which apparently do not. The latter cases fall into fairly definite groups but their nature is still obscure.

In the following pages it will be convenient to deal with the simpler cases first.

LYCHNIS.

The two forms *L. vespertina* and *L. diurna* were each intercrossed with white-flowered and red-flowered glabrous varieties. *L. vespertina* is a native of Cambridgeshire, and wild plants were therefore easily procured in the neighbourhood; *L. diurna* is not found in the county, and seeds of this form were collected in Cornwall. For the glabrous varieties, which were at the time growing in the Cambridge Botanic Garden, we are specially indebted to Professor de Vries.* All the forms are perennial.

The discontinuity between the hairy and the glabrous forms appears to be absolute; among the whole number of plants raised, not a single intermediate was observed. Such, however, is not the case when characters other than leaf surface are considered, as *e.g.*, colour of corolla (white or red), position of capsule teeth (erect or recurved), colour of seed (grey or black). When a cross was made between the types differing in respect of these latter characters, the offspring exhibited a series of intermediate forms. The corolla varied from the deep red of *diurna* to the palest pink, but was rarely, if ever, pure white; the capsule teeth might be directed obliquely upwards, as in *vespertina*, or be turned downwards and slightly recurved, or be in any intermediate position; but the rolled form characteristic of *diurna* was extremely rare. The seeds were of all tints from yellowish-grey to black.†

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* The discovery by de Vries of a wild specimen of *L. vespertina*, var. *glabra*, and the artificial production from it of a smooth red-flowered form are described in his 'Erfelijke Monstrositeiten," p. 10 (p. 26 of the French translation); he names this red-flowered form *L. diurna*, var. *glabra*. It may be noted, however, that such plants grown at Cambridge from his seed, although resembling *diurna* in the colour of the corolla, had not the recurved teeth characteristic of the capsule in that form; on the contrary, they were in this respect similar to the white-flowered form (*L. vespertina*, var. *glabra*) from which they were originally descended. The characters of hairiness and of the capsule-teeth are therefore dissociable.

† For a detailed description of hybrids of *L. diurna* and *L. vespertina* see Gagnepain, 'Bull. de la Soc. Bot. de France,' 1897, Tomes 43 and 44.

The results as regards hairiness are tabulated in the succeeding pages, in which the different forms are briefly indicated as follows:—

WH = *L. vespertina*, plant hairy, flowers white.
RH = *L. diurna*, plant hairy, flowers red.
WG = Glabrous variety with white flowers.
RG = ,, ,, ,, red ,,

In all cases the cotyledons are glabrous, but the seedlings can be sorted as soon as the next pair of leaves appear. In the glabrous varieties no hairs were observed upon any part of the plants at any time.

Table I.—First Cross-bred Generation obtained by crossing the Hairy and Glabrous Forms in each of the eight possible ways.

Parentage.*	Number of seed parents.	Number of flowers.	Number of offspring.	
			Hairy.	Glabrous.
WG × WH........	5	6	91	
WG × RH........	2	3	63	
RG × RH........	4	6	133	
RG × WH........	1	4	126	
WH × WG........	4	12	100	
WH × RG........	1	1	3	
RH × RG........	3	11	208	
RH × WG........	2	9	282	
Total...........	Individuals used more than once.	52	1006	0

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* Throughout the paper expressions in the form A × B indicate that A is the seed parent and B the pollen parent.

Table II.—Offspring of Cross-breds when fertilised *inter se.*[*]

Parentage.	Number of seed parents.	Number of flowers.	Number of offspring.		Totals for each particular kind of union.	
			Hairy.	Glabrous.	Hairy.	Glabrous.
(RH × RG) × (RH × RG)	1 1	2 1	17 82	6 21	99	27
(RH × WG) × (RH × WG)	1 1	1 1	31 11	12 4	42	16
(RG × RH) × (RG × RH)	1 1 1	2 1 2	47 62 28	17 14 10	137	41
(RG × WH) × (RG × WH)	1	1	59	14	59	14
(WG × WH) × (WG × WH)	1 1	1 2	15 37 7	6 16 2	59	24
(WG × RH) × (WG × RH)	1	1	12	4	12	4
Total	..	..	..	..	408	126

Expected result, 3 : 1; actual result, 3·2 : 1

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[*] As all the forms employed are diœcious, fertilisation of the cross-breds *inter se* is necessary where the alternative of self-fertilisation would be possible with hermaphrodite types.

Table III.—Offspring of Cross-breds when crossed with the original Recessive (Glabrous) Form.

Parentage.	Number of seed parents.	Number of flowers.	Number of offspring.		Totals for each particular kind of union.	
			Hairy.	Glabrous.	Hairy.	Glabrous.
(WH × WG) × WG	1	1	31	27	220	211
	1	1	67	60		
	1	2	77	75		
	1	2	45	49		
(WG × WH) × WG	1	2	18	25	18	25
(RH × RG) × RG	1	1	4	4	110	95
	1	2	16	11		
	1	3	90	80		
(RH × WG) × WG	1	1	9	7	39	42
	1	3	7	5		
	1	2	14	24		
			9	6		

Table V.—Offspring obtained by fertilising the Offspring of the Cross-breds *inter se*, (*α*) when both the final parents are hairy.
(*β*) „ „ „ „ glabrous.

Parentage.	Number of seed parents.	Number of flowers.	Number of offspring.	
			Hairy.	Glabrous.
(*α*.)				
[(WG × WH) × WG] × [(WG × WH) × WG]	1	1	23	6
[(RG × RH) × RG] × [(RG × RH) × RG]	1	1	87	25
Total	2	2	110	31
Ratio			3·5 : 1	
(*β*.)				
[(WG × WH) × WG] × [(WG × WH) × WG]	1	2		43
[(RG × RH) × RG] × [(RG × RH) × RG]	1	1		8
Total	2	2	0	51

A glance at the preceding tables will show—

(1) That the hairy character is dominant and the glabrous recessive (Table I). Of the thousand cross-breds raised from various unions between hairy and glabrous strains, all, without exception, were hairy.

(2) That such cross-breds invariably yielded both dominant and recessive forms if fertilised *inter se* (Table II), or if crossed with the recessive form (Table III); in the former case in the proportion of (about) 3 D to 1 R, in the latter of 1 D to 1 R; but that when crossed with the dominant form the offspring are again all dominant (Table IV).

(3) That when the offspring of cross-breds were intercrossed, like with like, the result varied according as they were of dominant or recessive form. Recessives fertilised *inter se* yielded only recessives (Table V*b*), but dominants (two individuals) similarly fertilised, yielded dominants and recessives in the ratio 3·5 : 1, thus proving impure dominants (dominant hybrids) which yield both forms in the succeeding generation; had either parent in either case been a pure dominant, the offspring would presumably have been all dominant (see Table IV).

These results clearly show that in the above-mentioned forms of Lychnis we have excellent illustration of a case to which Mendel's law is wholly applicable as regards the particular character of leaf surface.

ATROPA.

I. *First Cross-bred Generation.*

Reciprocal crosses were made between two individuals of *A. Belladonna typica* (flowers brown, fruits black, stem tinged with red = B), and a vigorous specimen of *A. Belladonna*, var. *lutea* (flowers and fruits yellow, stem pure green = Y).* Both forms are perennial.

From crosses of the form B × Y 100 plants were raised, and from the reciprocal unions Y × B, 236. In both sets of cross-breds the fruits (numbering in the three years, during which the plants have been under observation, some thousands) were all black, and though the flowers showed several shades of brown they never varied to pure yellow. The stems were more or less deeply tinged. It would appear then that the colour of the type form is completely dominant in the fruit of the cross-bred, but that in the corolla and stem the intensity of the dark coloration may be more or less diminished.†

II. *Offspring of Cross-breds.*

Twenty-five flowers on as many cross-breds were self-fertilised, but (in consequence perhaps of self-sterility) set no fruit; offspring were therefore obtained by fertilising the cross-breds with pollen from the parent forms these, all those with green stems bore yellow flowers and fruits, those with reddish stems brown flowers and black fruits, but the latter were more than twice as numerous as the former (197 B, 75 Y). In view of the earlier observation on the seedlings, however, it seems extremely doubtful whether any reliance can be placed upon the final numbers. Although, therefore, all that can be stated with certainty is that there is complete agreement with Mendel's law in so far—(1) that the fruit colour of the type form is completely dominant in the first cross-bred generation, whether the cross be of the form D × R or R × D; and (2) that the offspring resulting from a union between a cross-bred and a pure dominant are all dominant, but that when the cross is made with a pure recessive the offspring are mixed dominant and recessive; nevertheless, the observations made at the time of germination suggest that this agreement would probably prove quantitative as well as qualitative.

DATURA.

The following forms were employed—

1. *D. Stramonium* (stems green, flowers white, fruits prickly).
2. *D. Stramonium*, var. *inermis* (stems green, flowers white, fruits smooth).
3. *D. Tatula* (stems reddish, flowers violet, fruits prickly).
4. *D. Tatula*, var. *inermis* (stems reddish, flowers violet, fruits smooth).

Reciprocal crosses between these different types showed clearly that of the two pairs of antagonistic characters the violet colour is dominant, the white recessive, and similarly that the armed condition is dominant and the unarmed recessive; also that these results obtain whichever combination of forms is employed.

Several earlier observers have selected the various species of Datura as affording suitable material for the study of hybrids. Naudin* crossed *D. Stramonium* with *D. Tatula*, and *D. Tatula* with *D. Stramonium*, and found that in both cases the first generation of cross-breds resembled *D. Tatula*; in the second generation both the parent forms appeared, but *D. Tatula* was more abundant than *D. Stramonium*—the actual figures are not given. Similarly with *D. Stramonium* and *D. lævis*, the first generation resembled *D. Stramonium*, the second produced both forms. Later Godron's† results appeared with a record of the numbers obtained. This observer found that *D. Bertolonii*‡ crossed with *D. Tatula* yielded offspring all resembling *D. Tatula*; but that among the 29 plants obtained in the second generation three distinct forms occurred, viz., *Tatula*, *Tatula* var. *inermis*, and *Stramonium*, and in the proportion respectively of 16, 11, and 2. It will be noticed that with both observers *Tatula* was found to be a dominant form. As regards Godron's second result, a comparison with the following tables would seem to show that the numbers in the second generation are too small to afford conclusive evidence either as to the possible number of forms which may result from this particular union, or as to the relative abundance of each.

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* 'Nouv. Arch. Mus.,' 1, p. 41; for the earlier literature, see Focke's 'Pflanzen-Mischlinge,' p. 263.

† 'Mém. Acad. Stanisl.,' 1864, 1865, 1872.

‡ *D. Bertolonii* of Godron = *D. Stramonium*, var. *inermis*. *D. lævis* of Naudin is a white-flowered unarmed form not very different from this.

From two other experiments described by Godron it would appear that the offspring of cross-breds *may* exhibit a character which had not (presumably) been introduced by either of the original parents; *e.g.*, *Stramonium* × *Tatula* yielded in the second generation the four forms *Stramonium*, *Bertolonii*, *Tatula*, and *Tatula*, var. *inermis*, although neither of the original parents possessed unarmed fruits. Again from a cross between *Tatula* and *Tatula* var. *inermis*, he obtained in the second generation both the parent forms, and also *Bertolonii*, although both parents were of the dominant colour.

In each case it will be noticed that the unexpected form of offspring belonged to a recessive type. These results are at variance with those detailed below, but in the light of the facts which we now know, a possible explanation of these apparent contradictions at once suggests itself. May it not be that one (or both) of the crossed forms, although apparently pure, was in reality a cross-bred—an impure dominant—exactly resembling the pure dominant type which it was intended to employ? Godron states that the type forms were grown together in the same garden, and that he had been led to conclude that it was not necessary to cover the flowers in order to avoid chance cross-fertilisation. If then it is also borne in mind that a plant which in fruit-form, and in the colour of stem and flower has all the appearance of a specimen of *D. Tatula*, may, nevertheless, when self-fertilised, prove by its mixed offspring that it is not "pure" but a cross-bred derived from a cross with any one of the three forms *Tatula* var. *inermis*, *Stramonium*, or *Stramonium* var. *inermis*; it seems not impossible that such an unsuspected cross-fertilisation may have occurred among Godron's plants; for the effect of such a cross is not immediately apparent—not, in any case, until the next generation but one. These considerations suggest a very necessary precaution in all experiments of this kind, viz., the carrying out of control experiments in order to test the purity of the parent plants. When the immediate parentage of the individuals selected for crossing is unknown, the self-fertilisation of additional flowers would yield valuable evidence in corroboration (or otherwise) of the results obtained by cross-fertilisation. The importance of such controls in cases of complete dominance is obvious. The alternative hypothesis is, of course, that an original variation occurred.

Although the offspring resulting from a cross between any two of the forms employed are usually indistinguishable from the type which is dominant as regards the particular character crossed, yet in other cases the intensity of a dominant character may be more or less diminished either in particular individuals or in particular parts of one individual. In Tatula-Stramonium cross-breds the corolla is often paler in colour than that of the dominant parent (as has already been noticed by Naudin),[*] but even in the palest specimens the deep blue colour of the unopened anthers leaves no doubt as to the presence of the dominant colour element. When the seedling offspring of these cross-breds are sorted into groups according to the pure green or reddish-green colour of the hypocotyl, it was noticed that the tint in the red-stemmed individuals was not equally pronounced in all cases. The occurrence of intermediate forms was also occasionally noticeable in the fruits. Among the large number of capsules examined, there were some of the mosaic type, in which part of the capsule was prickly and the remainder smooth, while others, suggesting a blend, were more or less prickly all over, but the prickles were much reduced in size, and often formed mere tubercles. These mosaics occurred as rarities both on prickly individuals and on smooth ones still more rarely. Naudin observed the not uncommon occurrence of both these intermediate forms, but Godron[†] only records a single instance. Such intermediate fruits were most often found towards the end of the flowering season; occasionally, however, the first fruit of all was smaller than the others, and in that case often less prickly also. Increase in height and sterility of the flowers at the lower bifurcations occurred among the cross-breds, but to the slight extent observed by Godron, rather than in the more pronounced degree described by Naudin.

In one cross Godron obtained from *Stramonium* × *Bertolonii* plants all like *Bertolonii*, as stated. Among their offspring three forms appeared, *Stramonium*, *Bertolonii*, and *Tatula inermis*. These results are at variance with ours obtained from the reciprocal cross (*Stramonium inermis* × *armata*). Here the offspring resembled *armata*, and in the next generation only the parent-types *armata* and *inermis* came. Moreover, in no other experiment with these types has a dominant character (whether armed fruits or coloured flowers) appeared among the offspring of recessives. Yet in this one case both results are said to have occurred. Here the use of a DR by mistake for DD would not explain the contradiction, and we must suppose either that this is a true exception, or that error occurred in the experiment or description.

An analysis of the results of the various unions is shown in the following tables, in which the parent forms are indicated by the abbreviations T. ar., T. in., S. ar., S. in.

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[*] *Loc. cit.*

[†] *Loc. cit.*

I. *First Cross-bred Generation.*

Table VI.

Parentage.	Number of seed parents.	Number of flowers.	Number of offspring.			
			T. ar.	T. in.	S. ar.	S. in.
T. ar. × T. in. . .	2	3	28 23 17			
		Total	68	0	0	0
T. in. × T. ar. . .	1	1	28			
		Total	28	0	0	0
S. ar. × T. in. . .	1	1	61			
		Total	61	0	0	0
T. in. × S. ar. . .	1	1	41	1		
		Total	41	1	0	0
S. in. × T. ar. . .	3	3	43			

unarmed races are intercrossed the armed character is dominant, and that when Tatula and Stramonium forms are crossed together the dark colour of the former is also dominant.

II. *Offspring of Cross-breds.*

(*a.*) When Tatula *or* Stramonium cross-breds are self-fertilised.

Table VII.

Parentage.	Number of seed parents.	Number of flowers.	Number of offspring.					
			Number of seedlings.		Number of plants which matured fruits.			
			T.	S.	T. ar.	T. in.	S. ar.	S. in.
(T. ar. × T. in.) × self	3	3	62 31 30		19 18 24	1 6 5		
		Total	123	0	61	12	0	0
(T. in. × T. ar.) × self	2	2	49 34		19 all killed by frost.	6		
		Total	83	0	19	6	0	0
(S. in. × S. ar.) × self	4	4		54 49 10 8			27 34 6 5	16 5 2 2
		Total	0	121	0	0	72	25

Table VII shows that when cross-breds derived from unions between armed and unarmed races of *like* colour were self-fertilised, the offspring also were of one colour, uniform with that of the parents; but that as regards the fruits, they were in each case divisible into two groups, having respectively the types of capsule characteristic of the two parents. In two sets out of the three the totals approximate very closely to the expected ratio 3D : 1R, though individuals gave wide results; the remaining set, it will be noticed, includes one case in which there is a considerable discrepancy between the theoretical 3 : 1) and the observed (19D, 1R) result. The explanation possibly ies in the fact that more than two-thirds of the seedlings perished, and therefore the final numbers may not represent at all accurately the original proportion of the two forms. If this case be excluded, the agreement is fairly precise.

β.) When Tatula–Stramonium cross-breds are self-fertilised or fertilised *inter se*.

Table VIII.

Parentage.	Number of seed parents.	Number of flowers.	Number of offspring.					
			Number of seedlings.		Number of plants whic matured fruits.			
			T.	S.	T. ar.	T. in.	S. ar.	S.
r. × S. in.) × self or *inter se*	6	6	37	20	17	5	5	
			18	7	11	4	2	
			25	9	21	2	6	
			45	17	33	9	14	
			28	8	20	6	6	
			17	12	15	2	9	
		Total	170	73	117	28	42	
n. × T. ar.) × self	2	2	266	92	18	16	4	
			Not recorded.		22	9	14	
		Total	?	?	40	25	18	
n. × S. ar.) × self	6	7	58	28	2	1	—	

demanded by the theory, while that of the wholly recessive and least frequently occurring form S. in. is considerably below the calculated result. In considering the former discrepancy it should, however, be borne in mind that any errors in the classification of the many seedlings which perished early would all tend in the same direction, viz., to increase the Stramonium totals, since in the youngest seedlings the tinge of red is sometimes so faint that it may occasionally have escaped notice altogether; and regarding the latter, that in many cases the number of Stramonium plants which lived to maturity was decidedly small.

In the results recorded in Tables IX, X, and XI we have a much closer agreement with Mendel's ratio. Many of the cross-bred parents here used were among the number of those employed in the preceding experiments, where the expected result was less consistently approached, but there are manifestly more possibilities of fluctuation in the one case than in the other.

(γ.) When the cross-breds are crossed with the original recessive form

Table IX.

Parentage.	Number of seed parents.	Number of flowers.	Number of offspring.					
			Number of seedlings.		Number of plants which matured fruits.			
			T.	S.	T. ar.	T. in.	S. ar.	S. in.
(T. ar. × T. in.) × T. in.	3	3	56 36 19		23 19 8	24 13 11		
		Total	111	0	50	48		
Expected result	..	..	*All T.*		*1*	*1*		
(T. in. × T. ar.) × T. in.	2	2	45 ∞ (Almost all killed by frost)		6 5	7 —		
		Total	?	0	11	7		
Expected result	..	..	*All T.*		*1*	*1*		
(T. ar. × S. in.) × S. in.	6	6	Not recorded		9 8 6 3 1	10 6 6 3 1	8 6 11 5 —	6 7 6 3 2
		Total			27	26	30	24
Expected result	..	..			*1*	*1*	*1*	*1*

(δ.) When the cross-breds are crossed with the original dominant form.

Table X.

Parentage.	Number of seed parents.	Number of flowers.	Number of offspring.					
			Number of seedlings.		Number of plants whic matured fruits.			
			T.	S.	T. ar.	T. in.	S. ar.	S. i
(T. in. × T. ar.) × T. ar.	1	1	27		23			
Expected result	..	..	*All T.*		*All ar.*			
(S. in. × S. ar.) × S. ar.	1	1		51			34	
Expected result	..	..		*All S.*			*All ar.*	
(S. in. × T. ar.) × T. ar.	1	1	79		29			
Expected result	..	..	*All T.*		*All ar.*			

(ε.) When the cross-breds, whose parents each combined one dominant with o recessive character, are crossed with one of the parent forms.

Table XI.

Parentage.	Number of seed parents.	Number of flowers.	Number of offspring.					
			Number of seedlings.		Number of plants which matured fruits.			
			T.	S.	T. ar.	T. in.	S. ar.	S. i
(T. in. × S. ar.) × S. ar.	3	3	59 103 7	51 108 8	3 9 6		3 7 7	
		Total	169	167	18	0	17	0
Expected result	..	..	*1*	*1*	*All ar.*		*All ar.*	

III. *Subsequent Generations.*

Case 1. Parentage [(T. ar. × S. in.) × Self] × Self.
Assuming that Mendel's law holds,

T. ar. × S. in. gives all T. ar., impure, because

any such cross-bred (T. ar.) × Self gives a mixed offspring in the proportions 9 T. ar., 3 T. in., 3 S. ar., 1 S. in.

But of sixteen such individuals only one of each kind (four in all) is pure, yielding its like and only its like when self-fertilised ; from each of the twelve remaining plants (when self-fertilised) more than one type will be obtained. For

the 9 apparent T. ar. plants represent 4 different kinds of individuals, viz. :—

(*a*) 1 pure T. ar., yielding only T. ar.; indicated by the expression T. ar. (T. ar.).
(*b*) 2 impure T. ar., yielding T. ar. and T. in. in the ratio of 3 : 1; indicated by the expression T. ar. (T. in.).
(*c*) 2 impure T. ar., yielding T. ar. and S. ar. in the ratio of 3 : 1; indicated by the expression T. ar. (S. ar.).
(*d*) 4 impure T. ar., yielding T. ar., T. in., S. ar., S. in., in the ratio of 9 : 3 : 3 : 1 indicated by the expression T. ar. (S. in.).

the 3 apparent T. in. plants represent 2 different kinds of individuals, viz. :—

(*e*) 1 pure T. in., yielding only T. in.; indicated by the expression T. in. (T. in.).
(*f*) 2 impure T. in., yielding T. in. and S. in. in the ratio of 3 : 1; indicated by the expression T. in. (S. in.).

the 3 apparent S. ar. plants represent 2 different kinds of individuals, viz. :—

(*g*) 1 pure S. ar., yielding only S. ar.; indicated by the expression S. ar. (S. ar.).
(*h*) 2 impure S. ar., yielding S. ar. and S. in. in the ratio of 3 : 1; indicated by the expression S. ar. (S. in.).

the 1 S. in. is pure, yielding only S. in., indicated by the expression S. in. (S. in.).

Thus it is obvious that only in the case of those among the offspring of cross-breds (self-fertilised) which exhibit both recessive characters is it possible to know, without further experiment, with exactly what kind of individual one is dealing. In the following experiment a single flower was self-fertilised on each of eight individuals selected at random from among the offspring of cross-breds, viz., 3 T. ar., 4 T. in., and 1 S. ar. The results tabulated below afford a striking illustration of the Mendelian principle.

Plant.	Type.	Number of seedlings.		Number of plants which matured fruits.				Conclusions.
		T.	S.	T. ar.	T. in.	**S. ar.**	**S. in.**	
A	T. ar.	56		17	6			**∴ A = T. ar. (T. in.)**
B	,,	42	15	32		7		**,, B = T. ar. (S. ar.)**
C	,,	235	80	11	4	3	2	**,, C = T. ar. (S. in.)**
D	T. in.	27			10			**,, D = T. in. (T. in.)**
E	,,	15			4			**,, E = T. in. (T. in.) probably**
F	,,	23	7		20		5	**,, F = T. in. (S. in.)**
G	,,	8	2		8		1	**,, G = T. in. (S. in.)**
H	S. ar.		19			11	6	**,, H = S. ar. (S. in.)**

Case 2. Parentage [(T. ar. × S. in.) × S. in.] × Self.

In this case we should expect to find that only four kinds of individuals would occur among the offspring of the cross-breds (T. ar. × S. in.) resembling in appearance the four type forms, and therefore that all those of similar appearance would give similar

Case 3. Parentage [(T. ar. × S. in.) × S. in.] × S. in.

The offspring of any individual (T. ar. × S. in.) × S. in. should in this case exhibit the same number of forms as the offspring of any individual of similar appearance in the preceding experiment, but in the ratio, when mixed, of 1 : 1, instead of 3 : 1. The qualitative result was obtained, and, considering the small totals, the numbers may reasonably be taken to indicate a quantitative agreement also.

Plant.	Type.	Number of seedlings.		Number of plants which matured fruits.				Conclusions.
		T.	S.	T. ar.	T. in.	S. ar.	S. in.	
L	(S. ar.)		124			23	18	∴ L = S. ar. (S. in.)
N	(S. in.)		115				42	„ N = S. in. (S. in.)
O	(T. ar.)	43	35	15	11	9	18	„ O = T. ar. (S. in.)

Case 4. Parentage [(T. in. × S. ar.) × S. ar.] × Self.

Among the offspring of (T. in. × S. ar.) × S. ar., we find two forms only, viz., T. ar. and S. ar., but we may expect each form to include two kinds of individuals; there would thus be four kinds of individuals in all, of which one alone would be pure, and three impure, viz.,

(1) T. ar. yielding a mixed offspring in the proportions 9 T. ar., 3 T. in., 3 S. ar., 1 S. in.; ∴ T. ar. (S. in.).

(2) T. ar. yielding a mixed offspring in the proportions 1 T. ar., 1 S. ar.; ∴ T. ar. (S. ar.).

(3) S. ar. yielding a mixed offspring in the proportions 1 S. ar., 1 S. in.; ∴ S. ar. (S. in.).

(4) S. ar. yielding a pure offspring all S. ar.; ∴ S. ar. (S. ar.).

The nine individuals selected at random, viz., 4 T. ar. and 5 S. ar. justified expectation.

Plant.	Type.	Number of seedlings.		Number of plants which matured fruits.				Conclusions.
		T.	S.	T. ar.	T. in.	S. ar.	S. in.	
P	(T. ar.)	72	20	65		18		∴ P = T. ar. (S. ar.)
Q	„	100	30	45	14	14	5	„ Q = T. ar. (S. in.)
R	„	43	19	20	7	8	5	„ R = T. ar. (S. in.)
S	„	58	17	30	7	12	5	„ S = T. ar. (S. in.)
T	(S. ar.)		115			85		„ T = S. ar. (S. ar.)
U	„		13			10		„ U = S. ar. (S. ar.)
V	„		22			17		„ V = S. ar. (S. ar.)
W	„		193			69		„ W = S. ar. (S. ar.)
X	„		366			204	50	„ X = S. ar. (S. in.)

Case 5. Parentage [(T. in. × S. ar.) × S. ar.] × S. ar.

Five of the individuals used in the preceding experiment, viz., P, Q, R, T, U, were crossed with the pure form S. ar.; from T. U, the offspring would presumably be all S. ar., while from P, Q, and R alike, if the conclusions stated above are correct, we should expect the result 1 T. ar. : 1 S. ar. From each individual a result in accord with the expectation was obtained.

Plant.	Type.	Number of seedlings.		Number of plants which matured fruits.			
		T.	S.	T. ar.	T. in.	S. ar.	S. in.
P	(T. ar.)	68	74	45		52	
Q	(T. ar.)	67	58	40		35	
R	(T. ar.)	49	55	33		36	
	Total ..	184	187				
T	(S. ar.)		110			76	
U	(S. ar.)		69			63	
	Total ..	0	179				

List of Types used and their Characters.

The various races of stocks* employed differed in regard to

Habit, whether annual or biennial.
Character of leaf surface, whether hoary or glabrous.
Colour of flowers, various.†
Colour of seeds, whether green, brown, or intermediate.

Type 1. Biennial; leaves hoary; flowers white;‡ seeds black in outward appearance, this colour being due to green colour in the embryo overlaid with a violet layer; obtained from a coast-guard station in the Isle of Wight, and probably derived from the wild *M. incana.*

Type 2. Annual; leaves hoary on *under* surface, but glabrous, or nearly so, on the *upper* surface, a condition hereafter called "half-hoary"; flowers white; seeds yellowish brown; from same locality as Type 1. Origin unknown.

Type 3. Annual "ten-week" stocks; leaves hoary; flowers white;

3a. Seeds yellowish brown; kindly obtained by Mr. Lynch from the Royal Gardens, Kew.
3b. Seeds yellowish brown (commercial seed).
3c. Seed colour unknown, plants found in a Cambridge garden.

Of these three forms the first two, *3a* and *3b*, were indistinguishable in appearance, but behaved differently in crossing. Of *3c* very little was known.

Type 4. Annual "ten-week" stock; leaves hoary; flowers purple; seeds green (commercial seed).

Type 5. Annual "ten-week" stocks; leaves glabrous;§

5a.	Flowers white.	Seeds yellowish brown.
5b.	„ cream or yellowish white.	
5c.	„ red.	
5d.	„ pinkish white.	
5e.	„ bluish pink.	

(Commercial seed.)

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* Only single-flowered plants were chosen to breed from in any of these experiments. Doubles constantly presented themselves, but variations in respect of this character were not considered. Records were kept, but are not included in this paper. The same is true regarding the order of flowering of the axes. The character, width of seed-wing, used by Correns, was not studied.

† Unfortunately we have not yet fully identified the colour varieties according to the seedsmen's lists. Messrs. Haage and Schmidt have kindly supplied us with a set of their varieties, and from the descriptions and seed-characters we do not doubt that our "cream" is their "sulphur-yellow."

‡ In 1901 the flowers of this form were faintly tinged with purple.

§ In these forms the absence of hairs is absolute in many individuals, but in some cases a single hair or group of hairs may occur at the extreme apex of the leaf. The cotyledons of *all* types, whether glabrous or hoary, are invariably glabrous.

Type 6. Annual "ten-week" stocks; leaves glabrous;‡

6*a*. Flowers light purple.
6*b*. „ dark purple.
6*c*. „ claret coloured.
6*d*. „ lilac.
} Seeds green.
(Commercial seed.)

Type 7. Biennial "Brompton" stocks; leaves hoary; flowers white; seeds intermediate in colour, becoming much yellower on keeping.

7*a*.
7*b*.
7*c*.
} Samples from various sources. 7*b* was represented by a single plant.

Type 8. Biennial "Brompton" stocks; leaves hoary; flowers reddish purple; seeds green.

8*a*.
8*b*.
8*c*.
} Various plants of unknown origin which gave results not wholly uniform.

With the exception of 3*c*, 4, 7, and 8, which were not tested, all these varieties have been found to breed truly on self-fertilisation.

The colour of the seeds was found to be due to two distinct factors:

I. *First Cross-bred Generation.*

A. *Details as to Offspring of Hoary* × *Glabrous.* (*Table I.*)

From Table I it appears that when the hoary forms are employed as seed-parent, the hoary character is, except in one union (Experiment 10), completely dominant. Of about a 1000 individuals raised from such crosses between various hoary and glabrous strains, all were hoary. It is noticeable that when the form No. 2, which has only the underside of the leaves hoary ("half-hoary") was used, *the resulting plants were completely hoary like those bred from a completely hoary parent.* To this statement two individual offspring, which came of the half-hoary type, were the only exceptions out of more than 100 plants thus raised.

The exception to the rule of universal appearance of the hoary character is found in Experiment 10. In this experiment two sister plants belonging to the half-hoary type (2) when crossed with the glabrous form (5*a*) produced seventy-two plants all glabrous, although when crossed with another glabrous strain (6*c*) the same two individuals gave the usual result (Experiment 11). Now the sister plants were descended by two generations of self-fertilisation from a common grandparent, and both the glabrous strains with which they were crossed had also been raised by two successive self-fertilisations. Both the parent and the grandparent of the two half-hoary plants had, when crossed with the glabrous strains 5*b* and 6*a* respectively (see Experiments 7 and 1), produced the hoary form and the hoary form only, but whether the unusual result in the third generation is in any degree to be attributed to repeated self-fertilisation, the evidence to hand is not yet sufficient to determine. As against such a view, however, we have the result obtained in the experiment with the other glabrous form 6*c* (Experiment 11), in which no such effect was produced.

As regards seed-colour, green appears usually to be a dominant, brown a recessive character; but the intensity of the green colour may become so much diminished on crossing, that when seen through the brown testa, the net result may be a more or less intermediate tint.

As regards flower-colour the results were far more complex, the product of combinations of *identical* colours varying according to the particular strain employed. It will therefore be more convenient to treat the question of flower-colour when the results of the crosses glabrous × hoary detailed in Table II have also been considered.

B. *Details as to Offspring Glabrous* × *Hoary.*

From Table II it will be seen that in ten out of the fourteen fruits obtained in the first year (1897–8), from a cross between a *brown* seeded glabrous type and the *black* seeded hoary type 1 (Experiments cross Hoary ♀ × Glabrous ♂.

er s.	Colour of resulting seeds.	Number of offspring.			Colour of corolla in offspring.†
		Hoary.	Inter.*	Glabrous.	
	Green	34	—	—	Medium purple.
	Brown	16	—	—	Pink.
	Greenish	30	—	—	Medium purple.
	Brownish or greenish	48	—	—	Pink.
	Green	4	—	—	Medium purple.
	Green	21	—	—	Medium purple.
	Brown	47	1	—	Pink.
	Greenish	9	—	—	
	Brown	2	—	—	
	Brown	—	—	72	
	Green and brownish	117	1	—	Bluish pink.

ype No. 2, or exhibiting any lesser degree of hoariness; since in
ays hoary, it is evident that these individuals are more correctly
totals.
fore flowering.

Number of experiment.	Date.	Parentage.	Number of seed parents.	Number of flowers.	Colour of resulting seeds.	Number of offspring.			Colour of corolla in offspring.
						Hoary.	Inter.	Glabrous.	
12	1899–1900	[(3*b* × self) × self] × [(5*a* × self) × self] *H. wh. br.* × *G. wh. br.*	4	6	Green and few brown	194	—	—	Light purple.
13	..	[(3*b* × self) × self] × [(5*b* × self) × self] *H. wh. br.* × *G. cr. br.*	1	1	Inter.	3	—	—	Pink.
14	..	[(3*b* × self) × self] × [(6*c* × self) × self] *H. wh. br.* × *G. cl. gr.*	2	2	Green and greenish	13	—	—	Medium purple.
15	1900–01	1 × 5*b* *H. wh. bl.* × *G. cr. br.*	4	20	Green	371	—	—	
16	..	1 × 5*c* *H. wh. bl.* × *G. r. br.*	4	15	Green	119	—	—	
17	..	1 × 5*d* *H. wh. bl.* × *G. pk. wh. br.*	4	6	Green	77	—	—	
18	..	1 × 6*b* *H. wh. bl.* × *G. dp. gr.*	3	8	Green	145	—	—	

ABBREVIATIONS USED IN THE TABLES TO DENOTE LEAF-CHARACTER, FLOWER-COLOUR, AND SEED-COLOUR.

Leaves.—H, hoary; ½H, half-hoary; G, glabrous.

Flowers.—cl., claret; cr., cream; li., lilac; bl. pk., bluish pink; p., purple; dp., dark purple; mp., medium purple; lp., light purple; rp., reddish purple; r., red; wh., white; pk. wh., pinkish white.

Seeds.—bl., black; br., brown; gr., green; in., intermediate.

the cross Glabrous ♀ × Hoary ♂.

lour of resulting seeds.			Number of offspring.		Colour of corolla in offspring.	
reen.	Inter.	Brown.	Hoary.	Glabrous.	Hoary.	Glabrous.
All	—	—	16	—	Dark purple	
All	—	—	23	—	"	
All	—	—	19	—	"	
24 sown)			21	—	"	
	17		—	14		Cream.
		5	—	2		Cream.
All	—	—	63	—		
All	—	—	11	—	"	
All	—	—	13	—		
All	—	—	14	—	**Medium purple**	
30			26	—	"	
	1		—	1		**Pinkish.**
		6	—	6		**Pinkish.**
12 sown)			10	—	"	
		3				
28 sown)			25	—	"	
	4		2	—	**Produced no flowers**	
All	—	—	23	—		

Number of experiment.	Date.	Parentage.	Number of seed parents.	Number of flowers.	Colour of resulting seeds.			Number of offspring.		Colour of corolla in offspring.	
					Green.	Inter.	Brown.	Hoary.	Glabrous.	Hoary.	Glabrous.
23	1897-98	5*e* × 1 *G. bl. pk. br.* × *H. wh. bl.*	1	1	All ?	—	—	13	—	Medium purple	
				1	45 (26 sown)			23	—	,,	
							6 (4 sown)	—	4		Bluish pink.
24	,,	6*a* × 1 *G. lp. gr.* × *H. wh. bl.*	1	2	All (24 sown)	—	—	8	—	Light purple	
25	,,	6*b* × 1 *G. dp. gr.* × *H. wh. bl.*	1	1	All (12 sown)	—	—	12	—	Dark purple	
*25	1900–01	,, ,,	1	4	All	—	—	20	—		
				5	All	—	—	71	18		
26	1897–98	6*c* × 1 *G. cl. gr.* × *H. wh. bl.*	1	1	All	—	—	21	6	Dark purple	Claret.
27	,,	6*d* × 1 *G. li. gr.* × *H. wh. bl.*	1	1	All	—	—	22	—	Light purple	
28	,,	6*c* × 3*a* *G. cl. gr.* × *H. wh. br.*	1	1	All	—	—	11	—	Pink	
29	,,	6*d* × 3*a* *G. li. gr.* × *H. wh. br.*	1	1	All	—	—	14	—	Light purple	

19—23 without asterisks) the seeds were uniform in colour (green), and sample sowings yielded hoary plants only. In the four remaining fruits borne by some of the same individuals (Experiments 20 and 22) the seeds were mixed—green, intermediate (the embryos being either yellowish green, or green with yellow radicles), and brown—and gave rise to a mixed offspring. These green seeds, like those from fruits with green seeds *only*, gave rise to hoary plants, the brown seeds to glabrous plants, and the intermediates to both forms. In these experiments the hoary parent was *white* flowered. The glabrous parent was *either white*, *cream*, *red*, *or pinkish*. Nevertheless, irrespective of the colour introduced by the glabrous parent, all the *hoary* offspring were *purple* without exception. The glabrous offspring, however, invariably exhibited the colour of their glabrous parent.

When one individual belonging to each of the four *green* seeded glabrous strains was crossed with the same *black* seeded hoary type 1 (Experiments 24—27 without asterisks), the resulting seeds were of one type of colour, either pure or rusty green, and afforded no indication as to the nature of the future plants. A full sowing from one individual, and sample sowings from two others, produced forty-two plants all hoary; from the remaining individual a mixed offspring was obtained, in which as before the colour of the flowers of the glabrous plants resembled that of the glabrous parent. The hoary cross-breds parents were in each case descendants (by self-fertilisation) of the original plants.

In these unions of 1898–1900 (Table III), the occurrence among the offspring of a larger or smaller number of recessives (glabrous) was the rule rather than the exception. Complete prevalence of the hoary character was only obtained in the four experiments (30—33) in which the hoary parent was a purple-flowered Brompton stock, and in two out of the three cases in which the white-flowered Brompton form, 7*c*, was employed (Experiments 37 and 45). In the remaining cross with 7*c*, in all those with the other white-flowered Bromptons 7*a* and 7*b*, with the white-flowered annual 3*c*, and with the purple-flowered annual 4, the offspring included some recessives. Reference to the table, however, will show that in those cases in which the offspring were mixed, the mixed result was not given by every seed parent employed, but by particular individuals or even, indeed, by particular flowers. For in some cases in which several flowers on the same individual were crossed, the offspring of some were mixed and of others not. It is not surprising therefore that when the totals obtained in the different unions are compared, no constant proportion between the two characters is discoverable. So far, however, the irregularity of these results remains unexplained.

In regard to seed-colour, in the four experiments (42—45), where *green*-seeded glabrous strains were crossed with the white-flowered Bromptons (seeds intermediate) the results varied; twice the seeds were all green, once mixed, and once difficult to classify. When *brown*-seeded glabrous strains were crossed with the same white Bromptons (Experiments 35—41) the resulting seeds were never uniformly coloured, though occasionally all those in a single fruit were alike. Nevertheless, a comparison of the numbers failed to reveal any constant proportion between the various seed-colours. In the cases in which brown-seeded glabrous strains were crossed with purple-flowered forms (seeds green) we find the same irregular result as before in the cross with the annual type 4 (Experiment 34), but with the Brompton strains the case is very different (Experiments 30—32). In the unions with 8*b* and 8*c*, the green colour was completely dominant, with 8*a* equal numbers of brown and green seeds were produced, the colours being well-marked and without intermediates. This equality in the totals was further borne out by the figures obtained for each individual, but it is of interest to note that a similar uniformity of distribution did not always extend to the separate flowers. Although, as a matter of fact, every cross-bred fruit contained both seed forms, yet in some there were only one or two of one or other colour.

The occurrence here of both seed-colours recalls the mixed result obtained in some cases in the preceding year with the hoary type No. 1. In those cases, however, there was no constant proportion

Table III.—Offspring obtained from the cross Glabrous ♀ × Hoary ♂.

In these experiments the seed-parent was the result of one or two self-fertilisations.

No. of expt.	Date. 1898—99, 1899—1900	Parentage. .	Number of seed parents.	Number of flowers.	Colour of resulting seeds.			Number of offspring.*		Colour of flower in offspring.	
					Green.	Interm.	Brown.	Hoary.	Glabrous.	Hoary.	Glabrous.
30	. .	(5*b* × self) × 8*a* *G. cr. br.* × *H. rp. gr.*	8	4	105		101	98	—	Purple Red	
				2	46		57	83	—		
				4	99		89	45	—		
				2	51		59	46	—		
				2	21		20				
				4	49		55				
				2	40		37				
				1	4		8				
		Total. . . .	8	21	425	—	426	272	—		
31	. .	[(5*b* × self) × self] × 8*b* *G. cr. br.* × *H. rp. gr.*	4	4	All	—	—	26	—	Purple	
32	. .	[(5*a* × self) × self] × 8*c* *G. wh. br.* × *H. rp. gr.*	2	2	All	—	—	27	—	Purple	
33	. .	[(6*c* × self) × self] × 8*b* *G. ol. gr.* × *H. rp. gr.*	2	2	All	—	—	39	—		
34	. .	[(5*a* × self) × self] × 4	1	1	1	14	24	15	22	Purple	White

			1 {	3	8	75	—	46	—		
				1	—	—	35	34	1		
			3	12	65	—	484	260	16	Red	Cream
		Total....	12	35	294	146	850	672	17		
36	..	(5*b* × self) × 7*b* *G. cr. br* × *H. wh. in.*	1	1	3	17	11	13	8		Cream
37	..	[(5*b* × self) × self] × 7*c* *G. cr. br.* × *H. wh. in.*	2	5	93	71	65	155	—	Pink	
38	..	(5*a* × self) × 7*a* *G. wh. br.* × *H. wh. in.*	1	3	95	—	7	92	7	Purple	
39	..	(5*a* × self) × 7*b* *G. wh. br.* × *H. wh. in.*	7	18	483	34	63	440	27	Purple	
40	..	[(5*a* × self) × self] × 7*c* *G. wh. br.* × *H. wh. in.*	2	2	2	49	8	50	7		
41	..	(5*d* × self) × 7*a* *G. pk. wh. br.* × *H. wh. in.*	3	8	—	26	235	236	11	Pink	Pinkish white
42	..	(6*c* × self) × 7*a* *G. cl. gr.* × *H. wh. in.*	6	21	All	—	—	382	44	Purple	—
43	..	(6*c* × self) × 7*b*	3	12 {	∝			339	17	Purple	
		G. cl. gr. × *H. wh. in.*					39	—	38		Claret
		Total....	3	12	∞	—	39	339	55		
44	..	(6*b* × self) × 7*b* *G. dp. gr.* × *H. wh. in.*	1	1	All	—	—	31	8		
45	..	[(6*c* × self) × self] × 7*c* *G. cl. gr.* × *H. wh. in.*	3	3	Not classified			71	—		
46	..	[(5*b* × self) × self] × 3*c* *G. cr. br.* × *H. wh.* ?	2	2	96	2	4	33	4	Purple	Cream
47	..	[(5*a* × self) × self] × 3*c* *G. wh. br.* × *H. wh.* ?	2	2	59	—	4	57	2	Purple	White
48	..	[(6*c* × self) × self] × 3*c* *G. cl. gr.* × *H. wh.* ?	1	1	All	—	—	6	5	Purple	Claret

Generally speaking, when the seeds were mixed and gave rise to mixed plants, the glabrous plants arose from brown seeds.

* In this and all succeeding tables a blank space opposite the seed numbers indicates that these seeds were not sown.

between the two colours, and, on the other hand, brown seed-colour was correlated with glabrous leaf-character. It should, however, be stated that the seed-*colour* purity of the type 8*a* was not separately tested, as was done in the case of type 1, and that the production of *equal* numbers of the two seed-colours is precisely the result which we should expect if 8*a* were not a pure colour form, but itself a cross-bred in this respect. This possibility cannot now be disproved; but it may be noted that Trevor Clarke obtained a like result from a cross between a brown-seeded and a black-seeded form, 50 per cent. of the seeds coming black.

Finally in the remaining union (Experiment 33), in which both parents were green seeded, the resulting seeds were all green.

A further attempt to obtain confirmation of these results was made, especially in view of the appearance at this point of Correns'[*] "Levkojenbastarde." Commenting upon Trevor Clarke's results, which are at variance with his own observations regarding the complete dominance of the hoary character, he suggests that the occurrence of a mixed offspring in the first generation is to be attributed to subsequent (? self) fertilisation. Such an explanation, however, takes no account of the alteration in habit of the glabrous plants which Trevor Clarke describes, a fact which would seem to vindicate the accuracy of his experiments.[†] It seems equally unlikely that the exceptions to the of cases; but further experiments are needed in order to determine how far we are here dealing with a question of strain, of the individual, or even of separate flowers, or to what extent, if any, these results are dependent upon varying conditions.

Subsequent experiments, which will be set forth in detail hereafter, showed that the constitution of the dominant offspring, produced in these crosses, differed radically from that of the recessives. For while the former (hoary) were proved to give rise to mixed offspring in fair accordance with Mendelian expectation, the latter (glabrous) *behaved as pure glabrous forms, giving rise to glabrous offspring only, when self-fertilised.*

This phenomenon is discussed hereafter in connection with others of a like nature (p. 154).

C. *Flower-colour of First Cross-bred Generation.*

To return to the question of flower-colour, the results in both sets of experiments (A and B) may be most easily gathered from the following schedule, in which the various unions giving an identical result are grouped together in each column.

In regard to the recessive offspring such a schedule is unnecessary, since in each case the flower-colour of recessive offspring resembles that of the recessive parent, though whether the shade was identical in all cases is not certain.

In the table (p. 46), the crosses hoary × glabrous and glabrous × hoary have been separated for convenience of reference, but there is no reason to suppose any significance (in regard to flower-colour) to attach to the circumstance of the way the cross was made. Flower-colour was not a primary subject of these experiments, and, as it happens, no true reciprocals were flowered.

The coloured forms red, pink, purple, claret, have not yet been crossed *inter se.*

In each case one parent was white, except in one instance when cream was crossed with purple (13). *No white* offspring were produced in first crosses. In some cases there was obvious blending, as for example when white crossed with red or claret, gave pink. More often the result was a wholly new colour, and except in four cases (6, 9, 27, 13*b*), always some shade of purple, but varying in tint even in crossings of similar colours according to the particular strains employed. Not only are such purple offspring produced when some of the coloured forms are crossed with white, but even in the crosses *between two distinct white forms* (10, 19, 24). Possibly we have here a reappearance of the colour commonly occurring in the wild species (*incana*), from which the hoary garden stocks may have originated—a case in fact of "reversion" (see p. 144). In the four exceptions 6, 9, 27, 13*b*, in which cream was

INANT *Cross-breds.*

ary × Glabrous.

purple.	Pink.	Red.
	(5) white 2 × red 5*c*	
	(6) ,, × cream 5*b*	
	(7) ,, × claret 6*c*	
	(8) white 3*b* × red 5*c*	
	(9) ,, × cream 5*b*	
abrous × Hoary.		
6*a* × white 1	(25) pinkish white 5*d* × white 7*a*	(27) cream 5*b* × white 7*a*
× ,,	(26) claret 6*c* × white 3*a*	(13*b*) ,, × purple 8*a*
× white 3*a*		
a × white 7*b*.		

crossed with white or purple, we have the unexpected appearance of pink or red; a similar occurrence was observed by Correns among the offspring of cross-breds whose parents had "yellowish white" and "violet" flowers respectively.

In connection with the question of colour one point of interest may be briefly mentioned here, viz., the occurrence of "flaking." Although this phenomenon was occasionally observed in the coloured pure strains, even when repeatedly self-fertilised, it was not very common; among the coloured cross-breds, however, it was almost universal. Plants which at first produced exclusively self-coloured flowers, as the season advanced bore flowers speckled or streaked with white. In some cases the last flowers were even entirely white. Such flaking was recorded in thirty-one out of thirty-nine combinations in the first year, and in forty-one out of forty-three in the second.*

D. *Seed-colour of First Cross-bred Generation.*

When brown-seeded and green-seeded types (including "black") were intercrossed, green was generally dominant, though exceptions were seen. When browns were crossed with the intermediate type 7, the results were irregular. Similarly when greens were crossed with these intermediates the results were sometimes irregular. When browns were crossed with browns, the one being glabrous and the other hoary, the result was four times brown, once greenish, once intermediate, and twice a mixture. Green (including "black") with green always gave green.

When in first crosses *glabrous* plants were produced, these arose from seeds similar to those of the glabrous parent, except in one case where the intermediate type 7 was used. In that case glabrous plants came from both *green* and *brown* seeds.

II. *Posterity of Cross-breds.*

The offspring of cross-breds will be described in the following order:—

1. Offspring of cross-breds produced from (D × R).†

   A. *When D and R were brown-seeded and green-seeded respectively.*

      a. When the cross-breds were fertilised with a recessive form. (Table IV.)
      b. When the cross-breds were self-fertilised. (Table V.)
      c. Subsequent posterity derived from the foregoing. (Table VI.)

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* Such flaking is exceedingly common in the *late* flowers of Sweet Peas.

† In what follows, the classification into D, dominant, and R, recessive, refers to the characters hoariness and glabrousness, respectively.

B. *When D and R were both brown-seeded.*

- *a.* When the cross-breds were fertilised with a recessive form. (Table VII.)
- *b.* When the cross-breds were self-fertilised. (Table VIII.)
- *c.* Subsequent posterity derived from the foregoing. (Table IX.)

It was thought desirable to present the set of results contained in A separately from those in B, since the wholly unexpected results obtained in B (section *a*) were absent from the corresponding section in A.

2. Offspring of cross-breds produced from (R × D).

- *a.* When the cross-breds were fertilised with a recessive form. (Table X.)
- *b.* When the cross-breds were self-fertilised. (Table XI.)
- *c.* Offspring of dominant cross-breds produced in 2*a*. (Table XII and Table XIII.)
- *d.* When the cross-breds were fertilised with a dominant form.

Under these heads we have to consider the results of self-fertilising cross-breds (DR × self) and also the results of fertilising cross-breds with recessive (DR × R) and dominant (DR × D) forms. Subsequently we shall have to describe in each case experiments in which a well marked though not an absolute correlation between seed- and flower-colour. With few exceptions the pinkish flowers were derived from the brown seeds, and the purple from the green, so that as regards flower-colour the recessive offspring derived from brown seeds more nearly resembled their recessive parent, those from green seeds their more remote recessive progenitor.

In the parallel experiment with (3*b*), the seeds were uniformly coloured (green) but every individual produced a mixed offspring, though the final result (Experiment 50) was rather farther from the ratio 1 : 1.

b. *When the Cross-breds were Self-fertilised.*

The results recorded in Table V were obtained when the identical cross-breds employed as seed parents in Experiment 49 were self-fertilised instead of being crossed with a recessive form. The seeds, more difficult to sort than in the previous case, again showed a rough equality in the two type-colours, but the percentage of intermediates was much greater than before. From each individual and from each kind of seed a mixture of plants was obtained, the green and intermediate seeds giving rise to a majority of hoary, the brown to a majority of glabrous plants. The totals, however, showed a proportion of glabrous plants *far in excess* of the theoretical ratio of 1 : 3.

The above results will be considered in connection with those obtained in the next generation.

s (D × R) with a Recessive Form, when D and R were
n-seeded respectively.

ing	Number of offspring.			Colour of corolla in offspring.	
own.	Hoary.	Inter.	Glabrous.	Hoary.	Glabrous.
	gr. 12	—	*gr.* 11	Purple (various)	Purple (various) and pinkish.
35	*br.* 16	—	*br.* 8	Pinkish except 1 purple	Pinkish except 1 purple.
	gr. 10	—	*gr.* 13	Purple (various)	Purple (various).
	in. —	—	*in.* 3		
19	*br.* 12	—	*br.* 7	Pinkish	Pinkish.
	gr. 5	—	*gr.* 15	Purple (various)	Purple (various) and pinkish.
37	*br.* 18	—	*br.* 16	Pinkish	Pinkish.
	gr. 15	—	*gr.* 13		
39	*br.* 17	*br.* 1	*br.* 13		
	gr. 16	—	*gr.* 17	Purple (various)	Purple (various).
54	*br.* 21	—	*br.* 22	Pinkish	Pinkish.
	gr. 11	—	*gr.* 12		
24	*br.* 10	—	*br.* 13		
	gr. 14	—	*gr.* 15	Purple (various) and pinkish	Purple (various) and pinkish.
18	*br.* 6	—	*br.* 11	Pinkish	
	gr. 5	—	*gr.* 2	Purple (various)	Purple.
	in. 8	—	*in.* 7		

			3	66			gr. 31	—	gr. 31	Purple (various)	Purple (various).
		1				62	br. 40	br. 2	br. 23	—	Purplish-white.
			2	54			gr. 27	—	gr. 22	Purple (various)	Purple (various) except 1 white.
						41	br. 20	—	br. 19	Pink (various)	Pink (various).
		1	1	15			gr. 4	—	gr. 9		
						15	br. 7	—	br. 8		
			1	26			gr. 12	—	gr. 14		
		1				21	br. 9	—	br. 11		
			1	16			gr. 8	—	gr. 7		
						14	br. 6	—	br. 7		
			2	56			gr. 25	—	gr. 31	Purple (various)	Purple (various).
		1				47	br. 20	—	br. 26	Pinkish	Pinkish.
			2	25			gr. 11	—	gr. 13	Purple (various)	Purple (various).
						30	br. 11	—	br. 15	1 pinkish	1 white.
	Total…………	8	26	487	22	462	430	3	437		
	(3b × 6b) × (6c × self)	1	1	All	—	—	21	—	16		
50	(H.wh.br. × G.dp.gr.) × G.cl.gr.	1	1	„	—	—	20	—	12		
		1	1	„	—	—	18	—	14		
		1	2	„	—	—	50	—	30		
			3	„	—	—	39	—	47		
		1	1	„	—	—	14	—	19		
		1	1	„	—	—	38	—	42		
		1	1	„	—	—	18	—	17		
		1	4	„	—	—	90	—	61	Purple (various)	Purple (various).
		1	1	„	—	—	18	—	24		
		1	1	„	—	—	8	—	13		
	Total…………	10	17	All	—	—	334	—	295		

e Cross-breds (D × R), when D and R were brown-seeded respectively.

ing	Number of offspring.			Colour of corolla in offspring.		
Brown.	Hoary.	Inter.	Glabrous.	Hoary.	Inter.	Glabrous.
30	*gr.* 22 *in.* 32 *br.* 9	— *in.* 1 *br.* 5	*gr.* 13 *in.* 15 *br.* 16	Purple and Purplish-white. Purple and Purplish-white.		
55	*gr.* 25 *in.* 30 *br.* 10	— — *br.* 6	*gr.* 8 *in.* 15 *br.* 13	As the cross-bred and its recessive parent were both purple, there is no clear indication how the flower-colour descended. Absence of pure whites should be noted.		
41	*gr.* 39 *in.* 24 *br.* 6	— *in.* 1 —	*gr.* 5 *in* 11 *br.* 32			
58	*gr.* 34 *in.* 39 *br.* 6	— *in.* 2 —	*gr.* 20 *in.* 10 *br.* 24			
36	*gr.* 40 *in.* 39 *br.* 1	— — —	*gr.* 13 *in.* 17 *br.* 13			
52	*gr.* 35 *in.* 7 *br.* 15	— — *br.* 3	*gr.* 10 *in.* 3 *br.* 20			

No. of expt.	Parentage.	Number of seed parents.	Number of flowers.	Colour of resulting seeds.			Number of offspring.			Colour of corolla in offspring.		
				Green.	Inter.	Brown.	Hoary.	Inter.	Glabrous.	Hoary.	Inter.	Glabrous.
		1	4	58			*gr.* 30	—	*gr.* 10			
					30		*in.* 15	—	*in.* 11			
						46	*br.* 11	—	*br.* 24			
		1	3	48			*gr.* 21	—	*gr.* 22			
					41		*in.* 23	—	*in.* 13			
						38	*br.* 5	—	*br.* 32			
		1	2	21			*gr.* 12	—	*gr.* 4			
					—		—	—	—			
						47	*br.* 14	—	*br.* 17			
		1	?	225			*gr.* 121	—	*gr.* 39			
					112		*in.* 76	—	*in.* 16			
						241	*br.* 60	*br.* 22	*br.* 87			
		10	32 + ?	695	461	644	801	40	533			

c. *Subsequent Posterity derived from the foregoing (Table VI).*

In Experiments 52 and 53 (Table VI) we have results similar to those given in Table IV, the ratio of D to R being again not far from 1 : 1.

In Experiment 54, as in the comparable case in the preceding generation (Table V), we find that the number of glabrous plants is far too high; but that when the more hoary form 3*b* is substituted for the less hoary form 2 (Experiment 55), the result is exact—3D : 1R.

In Experiment 56, in which we should expect either 3D : 1R or else only dominants, according as the individual happened to be a DR or a pure D, it appears that three of the five individuals selected at random were DR and the remaining two pure D. Some of the offspring of the two latter plants, however, and also of one of the former, were half-hoary like the original hoary parent (type 2), or were even less hoary still, so that when quite young they might easily be mistaken for glabrous plants. Among the offspring of all the DR individuals there was the usual high percentage of recessives. In Experiment 57, a plant approaching the half-hoary character of type 2 (raised from (2 × 6) × self in Experiment 51) gave on self-fertilisation no offspring fully hoary, but only intermediate and glabrous plants.

Lastly, in the five remaining trials (Experiment 58), in which the

Table VI.—Experiments 52—58.

Number of experiment.	Parentage.	Number of seed parents.	Number of flowers.	Number of offspring.			Expected result.
				Hoary.	Inter.	Glabrous.	
	$[(D \times R_1) \times R_2] \times R_2$. Seed parent hoary.						
52	$[(2 \times 6a) \times (6c \times \text{self})] \times [(6c \times \text{self}) \times \text{self}]$	7	12	262	—	231	1D : 1R.
53	$[(3b \times 6b) \times (6c \times \text{self})] \times [(6c \times \text{self}) \times \text{self}]$	2	3	36	—	42	,,
	$[(D \times R_1) \times R_2] \times \text{self}$. Seed parent hoary.						
54	$[(2 \times 6a) \times (6c \times \text{self})] \times \text{self}$	7	34	312	8	162	3D : 1R.
55	$[(3b \times 6b) \times (6c \times \text{self})] \times \text{self}$	2	8	82	—	28	,,
	$[(D \times R_1) \times \text{self}] \times \text{self}$. Seed parent hoary.						
56*a*		3	3	33	—	14	3D : 1R, or all D, according as $[(D \times R_1) \times \text{self}]$ was DR or D.
	$[(2 \times 6a) \times \text{self}] \times \text{self}$		5	68	—	29	
			4	90	7	53	
56*b*		2	4	88	32	—	
			6	—	78	—	
	$[(D \times R_1) \times \text{self}] \times \text{self}$. Seed parent intermediate.						
57	$[(2 \times 6a) \times \text{self}] \times \text{self}$	2	5	—	17	58	No definite prediction as to the result of this fertilisation was possible, since the plant used was neither half hoary like the one original parent, nor glabrous like the other, but intermediate between the two.
	$[(D \times R_1) \times R_2] \times R_2$. Seed parent glabrous.						
58*a*	$[(2 \times 6a) \times (6c \times \text{self})] \times [(6c \times \text{self}) \times \text{self}]$	11	?	—	—	583	All R.
58*b*	$[(3b \times 6b) \times (6c \times \text{self})] \times [(6c \times \text{self}) \times \text{self}]$	2	2	—	—	56	,,
	$[(D \times R_1) \times R_2] \times \text{self}$. Seed parent glabrous.						
58*c*	$[(2 \times 6a) \times (6c \times \text{self})] \times \text{self}$	11	44	—	—	537	,,
58*d*	$[(3b \times 5c) \times (5a \times \text{self})] \times \text{self}$	2	6	—	—	9	,,
	$[(D \times R_1) \times \text{self}] \times \text{self}$. Seed parent glabrous.	3	8	—	—	56	,,
58*e*	$[(2 \times 6a) \times \text{self}] \times \text{self}$						

ds (D × R) with a Recessive Form, when D and R were
eeded.

ng	Number of offspring.			Colour of corolla in offspring.		
wn.	Hoary.	Inter.	Glabrous.	Hoary.	Inter.	Glabrous.
9						
17						
8						
35						
22						
44						
16						
16						
15						
50						
	23	—	—	Purple.		
19	—	—	19			White.

			3	42			41	—	—			
						42	—	—	41			
			2	40			39	—	—			
		1				44	—	—	42			
			1	9								
						10						
			1	16			16	—	—	Purple.		
						34	—	—	30			White.
			1	15								
						15						
		1	1	18								
						18						
			1	19								
						23						
		1	2	29								
						34						
	Total.........	8	26	406	8	471	119	—	132			
60	(2 × 5c) × (5b × self) (H.wh.br. × G.r.br.) × G.cr.br.	1	4	not classified			121	—	2			
		1	2	”	”		33	1	2	Red and bluish-pink	White	Pinkish.
		1	5	”	”		94	3	11			
	Total.........	3	11				248	4	15			
61	(3a × 5c) × (5a × self) (H.wh.br. × G.r.br.) × G.cr.br.	1	1	41	—	—						
		1	1	30	—	—	25	—	—			
		1	1	40	—	—	35	—	—			
			5	133 (20 sown)	—	—	17	—	—	Purple.		
					2		2	—	—			
						2	—	—	1			Red.
	Total.........	3	8	244	2	2	79	—	1			

ontinued.

ing	Number of offspring.			Colour of corolla in offspring.		
own.	Hoary.	Inter.	Glabrous.	Hoary.	Inter.	Glabrous.
	28	—	—	Purple (various).		
	2	—	—			
3	52	—	—	Red, except 1 pink.		
	22	—	—			
6	40	—	—			
	10	—	—	Purple.		
8 10 wn)	9	—	—	Red, pink, and bluish-pink.		
7	163	—	—			
—	23	—	—	Purple.		
	5	—	—	Purple.		
13 (5 wn)	3	—	2	Red, pink, and white		White.
	27	—	—			
1	1	—	—			
	11	—	—			
	25	—	—			
8	4	—	3			

		1	2	53	—	—	51	—	—			
		1	1	34			5	—	—			
					5		3	—	2			
						3	1	—	2			
	Total........	5	7	159	48	25	159	—	9			
64	(3*b* × 5*c*) × (5*b* × self) (*H.wh br.* × *G.r.br.*) × *G.cr.br.*	1	1	—	—	4	3	..	—	Red and pink.		
65	(2 × 5*c*) × (6*c* × self) (½ *H.wh.br.* × *G.r br.*) × *G.cl.gr.*	1	1	21			13	—	7			
						18	4	—	13			
		1	1	15			8	—	7			
						18	9	—	9			
	Total	2	2	36	—	36	34	—	36			

Sample sowings* (in each case of more than 100 seeds) further showed that the green seeds gave rise to hoary plants bearing purple flowers, a colour not found in either parents or grandparents, and the brown seeds to glabrous plants with white flowers like those of their recessive parent, and dominant grandparent. We may therefore conclude that the rough equality in the totals for seed-colour indicates in this case a corresponding agreement in the totals for the two kinds of plants differentiated as to flower-colour, and fair consistency with Mendel's ratio. There are indications that such correlation between the flower-colour and the leaf-character may sometimes, as here, be definite, though the experiments show considerable irregularities in this respect. (The 8 intermediates were not sown.)

In Experiment 60 in which (5*b*) was employed in place of (5*a*), the seeds though varying from brown to greenish were mostly of a nondescript intermediate tint difficult to classify. As the seeds could not be satisfactorily sorted all were sown, *and* 252 *hoary* (*including* 5 *intermediates*) *and only* 15 *glabrous plants* were obtained instead of 3 : 1. The latter plants bore pink flowers. Of the former some fifteen specimens were grown to maturity, and bore flowers varying from red to bluish-pink. Coincident with the absence of typical green seeds, we have the complete disappearance of the purple flowers characteristic of the corresponding offspring in the preceding experiment, and the

In Experiment 64 the offspring were too few (three) to give a decisive result.

In Experiment 62 the disappearance of the recessive form appears to be complete; *for we have the remarkable result that out of a total offspring numbering* 163, *not a single plant was glabrous*, though two of these seed-plants gave mixed offspring on self-fertilisation in Experiment 67. Finally, in Experiment 65, in which R_2 differs in seed-colour from R_1, we have a Mendelian result, both as regards seed-colour and leaf-character. These results will be considered in connection with the general statement of conclusions.

In regard to seed-colour the results were no less puzzling. In all cases the seeds were mixed. In the two experiments (59 and 65) in which a nearly Mendelian result was obtained, we find complete correlation between seed-colour and leaf-character in the former, and distinct evidence to the contrary in the latter. In Experiment 63 the correlation is imperfect; green seeds produced hoary plants, but brown seeds produced both forms. In Experiment 61 there is also some indication of correlation. In Experiment 62, on the other hand, the seed-colours afforded no clue, for all alike gave rise to hoary plants.

These results will be discussed hereafter. Attention must be directed to the fact that while on the one hand unions in the form $DR_1 \times R_2$ gave Mendelian results in both trials in which R_1 and R_2 were green-seeded, and in the one trial in which R_1 was brown-seeded and R_2 green-seeded; those in which R_1 and R_2 were both brown-seeded did so in one case only out of five. It is characteristic of these aberrant cases that there is an overwhelming preponderance of dominants. This discrepancy is certainly not fortuitous; *for either the one or the other result can be obtained from the same DR individual by an appropriate selection of the second recessive parent.* (See Experiments 85 and 87.)

What determines the several results is entirely unknown. R_1 differs from R_2, so far as we can tell, in nothing but colour.

b. When the Cross-breds were Self-fertilised.

Turning now to the results obtained on self-fertilisation (Table VIII), in Experiment 66, as in the previous case in which the half-hoary form (2) was used (Experiment 51), the glabrous offspring were far in excess of the Mendelian proportions of 1 : 3. Allowing for the possibility that seedlings which "damped off" very early, classified as glabrous, may, in reality, have been intermediates, the excess would remain considerable. The flowers of the glabrous offspring were white or pink, not red as in the recessive parent; those of the hoary plants white, pink, or red. The seeds were not classified.

In Experiment 67 there is a similar discrepancy between the expected and the observed results, 245D 131R instead of 3 : 1, although in this case the dominant parent (3*a*) was a fully hoary

Table VIII.—Offspring obtained from the Self-fertilisation of the Cross-breds (D × R) when D and R were both brown-seeded.

No. of expt.	Parentage.	Number of seed parents.	Number of flowers.	Colour of resulting seeds.			Number of offspring.			Colour of corolla in offspring.	
				Green.	Inter.	Brown.	Hoary.	Inter.	Glabrous.	Hoary.	Glabrous.
66	(2 × 5*c*) × self	1	4	Not classified			62	12	35	Red, white, and bluish-pink	Pink, white, and pinkish-white.
	(½ *H. wh. br.* × *G. r. br.*)	1	6	”	”		61	5	42		
		1	2	”	”		42	2	22		
		1	5	”	”		50	6	40		
		1	3	”	”		63	5	35		
		1	1	”	”		14	5	1		
		1	3	”	”		80	16	44		
		1	1	”	”		16	1	19		
	Total	8	25				388	52	238		
67	(3*a* × 5*c*) × self	1	1	6		24	5	—	1	Purple (various) Pink, white, and bluish-pink	Purple (various). Pink.
	(*H. wh. br.* × *G. r. br.*)						15	—	9		
		1	1	10		25	8	—	2		
							17	—	8		
		1	?	110	121	15	51	—	34		
							53	—	30		
							7	—	7		
		1	?	63			43	—	15		

No. of expt.	Parentage.	Number of seed parents.	Number of flowers.	Colour of resulting seeds.			Number of offspring.			Colour of corolla in offspring.	
				Green.	Inter.	Brown.	Hoary.	Inter.	Glabrous.	Hoary.	Glabrous.
68	(3*b* × 5*c*) × self	1	2	—	—	All	49	—	11		
	(*H. wh. br.* × *G. r. br.*)	1	1	—	—	”	30	—	9		
		1	1	—	—	”	21	—	8		
		1	?	—	—	”	31	—	9		
		1	?	—	—	”	221	—	81		
	Total	5	4 + ?	—	—	All	352	—	118		

form. The flowers of plants, whether glabrous or hoary, derived from green seeds, were purple; those from brown seeds, pink or more rarely white; on the other hand, there appeared to be *no* correlation between *seed-colour* and *leaf-character* in this particular case.

In Experiment 68 we have the exact result 3D : 1R. The seeds, unlike those in the two preceding experiments, were all of one colour, brown. The plants were destroyed before flowering.

The results in Experiments 66 and 68 confirmed those obtained in Experiments 51, 54, 55. The cross-breds *3b* × a recessive (whether green- or brown-seeded) gave a Mendelian result on self-fertilisation; whereas hybrids arising from the half-hoary type 2 × a recessive gave, on self-fertilisation, more than half the total offspring recessive. In Experiment 67—the case of cross-breds *3a* × *5c*—though *3a* is a form to all appearances similar to *3b*, the result of self-fertilisation is of the same exceptional character as that given by the cross-breds containing the half-hoary type.

c. Subsequent Posterity derived from the foregoing.

In Experiments 69—74 we have results obtained from a third crossing with a recessive form, when R_2 was again used as the recessive parent $(DR_1 \times R_2) \times R_2$. All the individual matings in four combinations, and seven out of eight matings in the fifth combination (Experi-

Table IX.—Offspring obtained from $[(D \times R_1) \times R_2]$.

a. When the seed parent is hoary and is crossed with R_2.
b. „ „ „ „ „ „ R_3.
c. „ „ „ glabrous „ „ R_2 or R_3.
d. „ „ „ hoary „ self-fertilised.

Number of experiment.	Parentage.	Number of seed plants.	Number of flowers.	Number of offspring.		Expected result.
				Hoary.	Glabrous.	
	a.					
69	[(2 × 5*c*) × (5*a* × self)] × [(5*a* × self) × self]	4	9	97	106	1D : 1R.
70	[(2 × 5*c*) × (5*b* × self)] × [(5*b* × self) × self]	4	8	138	138	„
71	[(3*a* × 5*c*) × (5*a* × self)] × [(5*a* × self) × self]	3	8	77	94	„
72	[(3*a* × 5*c*) × (5*b* × self)] × [(5*b* × self) × self]	5	7	114	57	„
73*a*	[(3*b* × 5*c*) × (5*a* × self)] × [(5*a* × self) × self]	7 (purple flowered)	10	133	131	„
73*b*		1 (red flowered)	2	21	—	„
74	[(3*b* × 5*c*) × (5*b* × self)] × [(5*b* × self) × self]	1	1	19	21	„
	b.					
75	[(3*a* × 5*c*) × (5*b* × self)] × [(5*a* × self) × self]	8	10	335	2	„
76*a*	[(3*b* × 5*c*) × (5*b* × self)] × [(5*a* × self) × self]	1	2	27	—	„
76*b*		1	1	24	23	„
77	[(3*b* × 5*c*) × (5*a* × self)] × [(5*b* × self) × self]	3	3	32	1	„
	c.					
78	[(2 × 5*c*) × (5*b* × self)] × [(5*b* × self) × self]	3	6	—	163	All R.
79	[(3*b* × 5*c*) × (5*a* × self)] × [(5*a* × self) × self]	2	3	77	1	„
80	[(3*b* × 5*c*) × (5*a* × self)] × [(5*b* × self) × self]	1	1	17	—	„
	d.					
81	[(3*a* × 5*c*) × (5*a* × self)] × self	2	10	140	89	3D : 1R, or all D, according as [(D × R₁) × R₂] was DR or D.
82	[(3*a* × 5*c*) × (5*b* × self)] × self	7	80	250	118	
83	[(3*b* × 5*c*) × (5*a* × self)] × self	3	several	146	65	
84	[(3*b* × 5*c*) × (5*b* × self)] × self	2	9	119	41	

Otherwise expressed:—

Individuals used as ♀s	in Experiment 73 gave a Mendelian result when crossed with R_2. in Experiment 77 gave an aberrant result when crossed with R_3.

also

Individual recessives used as ♂s in the third pollination	in Experiments 69, 71, 73 gave a Mendelian result. in Experiments 75 and 76 gave an aberrant result. also in Experiments 70 and 74 gave a Mendelian result. in Experiment 77 gave an aberrant result.

Therefore the difference between the results can scarcely be attributed merely to the individual constitutions of either parent, but must be due to some relation subsisting between the two.

The fewness of recessives occurring among the offspring in the cross with a cream-flowered recessive), and in Experiment 76, the seeds were not easily classified in regard to colour.

As regards Experiments 78—80, where the final seed parent was glabrous, we have a normal Mendelian case in 78, the offspring being all glabrous. But in 79 and 80 the result is puzzling and entirely inexplicable upon the Mendelian theory, for these two unions between plants of the recessive form gave offspring which, with a solitary exception, *were all dominants.* It may be added that the two final seed parents from which this unexpected result was obtained were sister plants to the exceptional individual previously mentioned in Experiment 73 as having given *all dominants* when crossed with a recessive. (See Conclusions, p. 86.)

The three exceptional plants arose from a cross between a cross-bred and an individual pure recessive (5*a*) which was used as pollen parent in several parallel experiments giving normal results. That cross gave both green and brown seeds, the three exceptional plants (one hoary and two glabrous) all arising from the latter. Sister plants arising from the green seeds were all hoary, and three of them contributed to the Mendelian result in Experiment 73.

Fortunately the two exceptional glabrous plants were also self-fertilised, and the result in this case was precisely the same as in all other cases of self-fertilisation of individuals of the recessive form—the offspring were all glabrous (see p. 79). But for this result one might have been tempted to suppose that some error had occurred in recording the leaf-character in these two plants. It seems equally improbable that in the final cross any such mistake as the unintentional taking of pollen from a hoary individual similar in colour to the recorded glabrous parent can have been made. Such an explanation would still leave the result in Experiment 80 unexplained, since at the time that this experiment was made, no *cream*-flowered *hoary* plants had ever been seen. These two cases, then, remain as the solitary instances among all the various unions that were made, of the occurrence of *dominant* offspring when both parents belong to a *recessive* type.

The results of Experiments 81—84, in which hoary cross-breds of the form $(DR_1 \times R_2)$ were self-fertilised, are on the whole confirmatory of those obtained from the self-fertilisation of the (DR_1) cross-breds from which they were derived. In the two cases in which the dominant ancestor was the hoary form 3*a* (Experiments 81, 82), the proportion of recessive offspring obtained from the $(DR_1 \times R_2)$ cross-breds was, as in the case of the (DR_1) cross-breds, considerably in excess of the theoretical result 3D : 1R. When 3*b* was employed as the dominant parent in place of 3*a* (Experiments 83, 84), we get a Mendelian result (3D : 1R) from two cross-breds of the form [(3*b* × 5*c*) × 5*b*], as we did from their cross-bred ancestors (3*b* × 5*c*). On the other hand, from two of the three cross-breds of the form $[(3b \times 5c) \times 5a]$, which contributed to the total result 146D 65R (in Experiment 84), the proportion of recessives is a good deal too high.

2. *Offspring of Cross-breds produced from* $(R \times D)$.

a. *When the Cross-breds were Fertilised with a Recessive Form (Table X).*

a. In Experiment 85, in which the cross-breds $(5a \times 1)$ were crossed with a recessive of the same type as the recessive parent $5a$, every fruit contained green and brown seeds in very varying proportions, and usually a few intermediates. All the intermediates (16) were sown, and glabrous plants only were obtained from them; samples of green and brown seeds gave all hoary (33) and all glabrous (16) plants respectively. Correlation between leaf-surface and seed-colour was apparently complete. On the assumption that all the green seeds would have produced hoary plants, and all those not green, glabrous plants, the offspring from every fruit would have been mixed, and the total numbers would have been in the proportion 228 hoary and 191 glabrous, as compared with the expected result 1D : 1R.

In Experiment 86, in which the cross-breds $(5b \times 1)$ were crossed with a recessive of the same type as the recessive parent $5b$, every fruit contained seeds of the three colours. None of the colours were both *green* in seed-colour, green and intermediate coloured seeds were produced, but not brown. Both occurred in every fruit, and both gave rise to hoary and glabrous plants, the totals in the case of both seeds and plants being fairly consistent with Mendel's ratio (82 green, 70 intermediate : 64 hoary, 76 glabrous).

These remarkable results are in harmony with those obtained from the (D × R) cross-breds (Table VI), and show that when the same recessive strain is introduced in each successive cross, Mendel's law holds; but that the successive use of recessive strains of different colour may exert some disturbing effect upon the numerical relation of the two kinds of offspring—an effect of which the data at present to hand afford no explanation. (See Conclusions, p. 86.)

b. *Offspring obtained when the Cross-breds were Self-fertilised.*

In Experiment 92 (Table XI) the cross-breds (5*a* × 1), of which several were among the number of those previously used for Experiments 85, 87, were self-fertilised. Every cross-bred produced green, brown, and intermediate seeds, the proportion of green to non-green being roughly 3 : 1, except in one mating, in which only one fruit was gathered. Sample sowings showed complete correlation between seed-colour and leaf-characrer, green seeds giving rise to hoary, brown and intermediate to glabrous plants. Presumably, therefore, the total number of plants would have been in the proportion 1244 hoary and 407 glabrous—a very close approximation to the Mendelian ratio 3 : 1.

In Experiment 93 cross-breds (5*b* × 1), several of which had been previously used for Experiments 86 and 88, were self-fertilised. The resulting seeds were mixed, but in very varying proportions. The number of intermediates was large—nearly one-fourth of the total number. Of the samples sown, green seeds again produced hoary plants; brown seeds, with four exceptions, glabrous plants, unless these four seeds should more properly have been included among the intermediates. The intermediates, however, yielded both forms, and it was therefore necessary to sow the whole number. Unfortunately, a large proportion—more than 100—failed to germinate. If, however, we may suppose that no considerable error is due to this fact, then, assuming perfect correlation in the case of the two other colours (four seeds excepted), the total number of plants would have been in the proportion 1429 hoary, 373 glabrous, or as 3·8 : 1.

In each of the three following experiments (94—96) only a single cross-bred was self-fertilised. The seeds were difficult to sort, and a full sowing was therefore necessary. In the case of the cross-bred (5*d* × 1) (Experiment 94), an exact Mendelian result was obtained (117D, 39R). With the cross-breds (5*e* × 1) and (6*c* × 1), however, the numbers showed a considerable deviation from the expected result—98 D *to 18 R in the* former case, 175D to 39R in the latter.

Cross-breds (R × D) with a Recessive Form.

ting seeds.		No. of offspring.		Colour of corolla in offspring.	
.	Brown.	Hoary.	Glabrous.	Hoary.	Glabrous.
		gr. 21	—		
		—	*in.* 3		
	43	—	*br.* 11		
		gr. 12	—	Purple.	
		—	*in.* 1		
	12 (5 sown)	—	*br.* 5		White.
		—	—		
		—	*in.* 5		
	29	—	—		
		—	—		
		—	*in.* 2		
	21	—	—		
		—	—		
		—	*in.* 2		
	18	—	—		
		—	—		
		—	—		
	43	—	—		
		—	—		
		—	*in.* 3		
	9	—	—		
	175	33	32		

	Total	7	[illegible]	[illegible]	[illegible]	[illegible]	[illegible]			
86	(5*b* × 1) × (5*b* × self) (*G.cr.br.* × *H.wh.bl.*) × *G.cr.br.*	1	8	75	31	42				
		1	5	51	82	51	*gr.* 5 *in.* 14 *br.* 12	*gr.* 3 — *br.* 22	Purple, red	Cream, white.
		1	3	77	51	76	*gr.* 37 *in.* 27 *br.* 10	— *in.* 15 *br.* 65	Red	Cream, white.
		1	2	41	34	59	*gr.* 8 *in.* 28 —	— *in.* 5 *br.* 53		
		1	2	48	39	65	*gr.* 39 *in.* 21 *br.* 28	— *in.* 4 *br.* 52		
		1	2	49	46	44	*gr.* 25 *in.* 31 *br.* 1	— *in.* 9 *br.* 43	Purple.	
		1	1	19	15	29	*gr.* 19 *in.* 12 —	— *in.* 3 *br.* 28	Purple.	Cream, white. Cream, white.
	Total............	7	18	360	298	366	297	302		
87	(5*a* × 1) × (5*b* × self) (*G.wh.br.* × *H.wh.bl.*) × *G.cr.br.*	1	2	97	—	—				
		1	1	58	—	—	*gr.* 40	—		
		1	1	56	—	—	*gr.* 47	—		
		1	1	57 (12 sown)	1	6	*gr.* 11 — —	— *in.* 1 *br.* 1	Purple.	White. White.
	Total...	4	5	268	1	6	98	2		
88	(5*b* × 1) × (5*a* × self) (*G.cr.br.* × *H.wh.bl.*) × *G.wh.br.*	9	13	∞ (98 sown)	7	2	*gr.* 73 *in.* 5 —	— *in.* 1 *br.* 2	Purple (various).	White.
	Total............	9	13	∞	7	2	78	3		

ntinued.

ting seeds.		No. of offspring.		Colour of corolla in offspring.	
.	Brown.	Hoary.	Glabrous.	Hoary.	Glabrous.
	—	23	—	Purple.	
		gr. 1	—		
		in. 11	—		
	17	*br.* 17	—		
		gr. 14	—		
		in. 18	—		
	85	*br.* 21	—		
	102	82	—		
		gr. 21	*gr.* 17	Purple (various) White, pinkish-white	Purple (various). White, pinkish-white.
		in. 9	*in.* 15		
		gr. 4	*gr.* 8		
		in. 2	*in.* 7		
		gr. 10	*gr.* 4		
		in. 9	*in.* 12		
		gr. 3	*gr.* 5		
		in. 6	*in.* 8		
	—	64	76		

Table XI.—Offspring obtained from the Self-fertilisation of hoary Cross-breds (R × D).

No. of expt.	Parentage.	No. of seed parents.	No. of flowers.	Colour of resulting seeds.			No. of offspring.		Colour of corolla in offspring.	
				Green.	Inter.	Brown.	Hoary.	Glabrous.	Hoary.	Glabrous.
92	(5*a* × 1) × self (*G.wh.br.* × *H.wh.bl.*)	1	6	270	26	57	—	*in.* 16		
		1	3	152	3	41	—	*in.* 3		
		1	1	58	2	5	—	*in.* 1		
		1	1	47	3	11	—	*in.* 1		
		1	3	139	9	32	—	*in.* 5		
		1	5	229	15	71	—	*in.* 11		
		1	4	177	12	54				
		1	4	172 (59 sown)	6	60 (32 sown)	*gr.* 59 — —	— *in.* 5 *br.* 32	Purple.	White.
	Total........	8	27	1244	76	331	59	74		

continued.

of resulting seeds.		No. of offspring.		Colour of corolla in offspring.	
Inter.	Brown.	Hoary.	Glabrous.	Hoary.	Glabrous.
13	16	*gr.* 12 *in.* 8 *br.* 1	— *in.* 2 *br.* 10	Purple.	Cream, white.
46	54 (12 sown)	*gr.* 10 *in.* 25 —	— *in.* 1 *br.* 11	Purple.	Cream, white.
		gr. 10	—	Purple, except 1 pink.	
25	—	*in.* 12 —	*in.* 7 —		White.
44	65 (12 sown)	*gr.* 12 *in.* 29 —	— — *br.* 11	Purple.	White.
23	9	*in.* 15 —	*in.* 6 *br.* 9		
20	11	*in.* 14 —	*in.* 2 *br.* 10		
13	8	*in.* 11	—		

		1	1	47	19	7	*in.* 13	*in.* 4		
		1	1	38	20	11	*in.* 17 *br.* 1	— *br.* 10		
		1	4	168	86	30	*in.* 44	*in.* 8		
		1	1	37	14	14	*in.* 10 —	*in.* 5 *br.* 14		
		1	2	72	30	24	*in.* 26	—		
		1	1	47	5	17	*in.* 1 *br.* 2	— *br.* 7		
		1	1	34	27	16	*in.* 18	*in.* 3		
		1	2	77	28	21	*in.* 16	*in.* 2		
		1	1	34	19	18	*in.* 15	—		
	Total..........	14	19	1049	432	321	317	122		
94	(5*d* × 1) × self (*G.pk.wh.br.* × *H.wh.bl.*)	1	?	Not classified			117	39	Purple	White.
95	(5*e* × 1) × self (*G.bl.pk.br.* × *H.wh.bl.*)	1	?	„ „			98	18	Purple, white	Purple, white.

continued.

of resulting seeds.		No. of offspring.		Colour of corolla in offspring.	
Inter.	Brown.	Hoary.	Glabrous.	Hoary.	Glabrous.
Not classified.		175	39	Purple, white, pink	Purple, white, pink.
	8 12	*gr.* 20 *br.* 6 *gr.* 20 *br.* 7	*gr.* 9 *br.* 2 *gr.* 10 *br.* 4	Pink, bluish-pink White	Pink, bluish-pink. White.
—	20	53	25		

In Experiment 97, in which the cross-breds (*6c* × *3a*) were self-fertilised, green and brown seeds were obtained in the proportion of 3 to 1. There was no correlation between seed-colour and leaf-character, for each colour gave rise to both kinds of plants. The totals 53D and 25R show too high a proportion of the glabrous form, a result which entirely agrees with that obtained from the self-fertilisation of the other set of cross-breds in which the hoary type *3a* was used as the dominant parent (Experiment 67).

From (R × D) × self, therefore, close Mendelian results were obtained in three out of the five cases in which the D parent was of the hoary type 1, viz., in those in which the R parent had white, cream, or pinkish-white flowers; but when the R parent had bluish-pink or claret-coloured flowers, there was a deficiency of R offspring. On the other hand, when the type *3a* was used as the D parent, there was too high a proportion of R plants.

c. *Offspring of Dominant Cross-breds produced in 2a (Tables XII and XIII).*

In accordance with the Mendelian hypothesis, all dominant cross-breds resulting from the cross (R × D) × R, are of the class of impure dominants, and when self-fertilised, yield D and R in the proportion of 3 to 1. We should therefore expect all the final seed parents in Experiments 98—102 (Table XII) to yield a mixed offspring in which the proportion of hoary to glabrous is as 3 to 1. On the other hand, dominants resulting from (R × D) × self are by theory of *two* kinds—(1) impure dominants as above, and (2) pure dominants, which, when self-fertilised, yield dominants and dominants only. In Experiments 103—107 (Table XIII) we should therefore expect the results to vary, according as the final seed-parents selected at random belonged to the former or the latter class.

In Experiments 98 and 99, in which cross-breds of the form [(R_1 × D) × R_1] were self-fertilised, there is close agreement between the observed and the theoretical result, 3D : 1R.

In Experiments 100—102 (Table XII), in which cross-breds of the form [(R_1 × D) × R_2] were self-fertilised, we find that some of the cross-breds give the expected result 3D : 1R; others on the contrary yielded *only dominants.*

This fact is the more surprising since such cross-breds giving one a Mendelian and another an aberrant result, may be sister plants, derived by self-fertilisation from the same parent.

In Experiments 103—107, in which fourteen individuals selected at random from among the dominant offspring of various self-fertilised (R × D) cross-breds were self-fertilised, eight were evidently pure dominants, and produced only dominants, six were impure dominants, isation of [(R × D) × R] when it is Hoary.

Number of flowers.	Number of offspring.		Expected result.
	Hoary.	Glabrous.	
18	298	95	3D : 1R.
12	364	113	
?	105	—	
?	73	29	
24	348	110	
5	94	—	
5	68	17	

isation of [(R × D) × self] when it is Hoary.

Number of flowers.	Number of offspring.		Expected result.
	Hoary.	Glabrous.	
12 or more	374	141	3D : 1R, or all D, according as [(R × D) × self] was DR or D.
8 or more	372	—	
12	190	—	
2	101	—	
2	12	3	
1	48	—	
4	63	14	
8	70	—	

and yielded a mixed offspring, in one experiment 374D, 141R, in the second 12D, 3R, and in the third, 62D, 14R.

d. *When the Cross-breds were Fertilised with a Dominant Form.*

When some of the various (R × D) cross-breds used in the preceding experiments were crossed with the same hoary white-flowered Brompton Stocks, (7*a* and 7*b*), used in the experiments described in Table II, the resulting offspring (148) were, in accordance with expectation, all hoary.

III. *Purity of "Extracted" Recessives and Dominants.*

According to the Mendelian hypothesis, such recessives are pure, and should behave as though they were of unmixed descent. If this be the case, they should, when self-fertilised, produce recessives and recessives only. To test this particular point, many such extracted recessives were self-fertilised, and thus produced more than 1000 offspring, all of which were recessive, so far confirming the anticipation as to their purity.

At the same time we have the contradictory result mentioned in Experiments 79 and 80, that from two such extracted recessives crossed with a *pure* recessive (the product of two generations of self-fertilisation), 94 *dominants were obtained and a single recessive.* This result is unexplained.

Purity of extracted *Dominants* was shown in Experiments 103*b*, 104, 105, 106*b*, 107*b*.

IV. *Nature of Recessives produced in the First Crosses.*

These first-cross recessives, when self-fertilised for two generations, invariably gave recessive offspring, having flower- and seed-colours identical with those of the original glabrous parent. The number thus produced, by several individuals, amounted to more than 200 in the first generation and about 130 in the second.

The question whether such recessives can have been the result of accidental self-fertilisation was considered (p. 44), and at present we strongly incline to think that they were not, while fully recognising the difficulties this view entails.

V. *Time of Flowering of Cross-breds.*

Of the original types, some were annual, some biennial.

Only a limited number of observations were made upon the inheritance of these qualities, owing to the difficulty of sowing and planting such large numbers simultaneously. The biennial forms (viz., *incana* (type 1), and the various Brompton strains), do not bloom in the first season, but in the second year they usually flower some weeks before the annuals. *With rare exceptions, all cross-bred plants and "extracted" types flowered in the first season.* The offspring of various crosses between annual forms flowered at the same time whether they were hoary or glabrous; but among the offspring of various crosses between hoary biennials and glabrous annuals it was usual for the recessives to flower before the dominants. In fact, the difference in time in some cases was sufficient for the former to be almost over before the latter had well begun. A certain number of these extracted recessives, however, flowered simultaneously with the dominants obtained from the same mating. Very rarely such recessives produced no flowers at all in the first year: in such cases the habit of growth of these plants resembled that of the biennial forms.

VI. *Summary of Conclusions from Experiments with Matthiola.*

The results obtained are so complex that it is difficult to draft statements which shall give a precise and comprehensive view of the phenomena.

Generally speaking, the following rules hold good:—

1. In crosses between hoary and glabrous plants, the former character was dominant, the latter recessive.

2. Such cross-breds, whether self-fertilised, or fertilised with a

7. Crosses between the half-hoary type and recessives gave in the first generation fully hoary cross-breds with only two half-hoary exceptions. Of the mixed offspring derived from these fully hoary cross-breds the great majority of dominants were again fully hoary; some, however, were half-hoary, and some intermediate between the half-hoary and the glabrous forms. No line could be drawn between such intermediates and the half-hoary plants, and therefore all are classed together as "intermediate" in the tables. It is possible that the fully hoary cross-breds are DR, and the others DD, but the numbers do not accord well with this simple explanation.

8. Crosses between white-flowered hoary and variously coloured glabrous forms commonly gave purple-flowered offspring (sometimes, however, a blend, *e.g.*, pink from red and white). This fact, and that given in 7, indicate that the first-cross offspring sometimes present atavistic characters. Crosses between different *coloured* pure types were for the most part not made (see p. 46).

9. The results of self-fertilising cross-breds produced by parents of dissimilar colour was usually to reproduce the parental flower-colours, accompanied sometimes by colours not seen in the parents, together with the purple or pink form of the first cross when either occurred.

10. Crosses between parents of different seed-colour, brown and green, showed green to be frequently dominant. Deviations from this general result occurred with certain types used.

11. In certain combinations there was close correlation between (*a*) green colour of seed and hoariness, (*b*) brown colour of seed and glabrousness. *In other combinations, such correlation was entirely wanting.*

12. The rule that plants with flowers, either purple or claret, arose from green seeds was universal. For seed-colours of the pure white types, see pp. 33 and 34.

13. When glabrous plants were produced by the first cross, they were found to be pure recessives, and their colour was that of their recessive parent. Such plants were always the offspring of recessive *seed* parents, and there is therefore suspicion of self-fertilisation. But with the most punctilious care, such recessives repeatedly occurred when certain strains were used, and in crosses with other strains they were uniformly absent.

Taking all evidence together, we incline to believe that these glabrous first-cross offspring were examples of the "false hybridism" of Millardet (see p. 154).

14. The fact stated in 3, that in certain subsequent crosses individuals occurred which were apparently pure dominants, seems to indicate that similar "false hybrids" may occur of pure *dominant* form also.

15. Speaking generally, the results here detailed agree well with those of Correns. But owing to the use of a large number of types,

our results are necessarily much more complex. It should also be noticed that none of our experiments are precisely comparable with that of Correns, who used a biennial hoary type with purple flowers and a biennial glabrous type with flowers "yellowish-white" (or "sulphur-yellow" of Haage and Schmidt). We used no *biennial* glabrous form.

16. In regard to almost every character studied, the behaviour of several of the different types, strains, and even individuals showed special peculiarities.

The reader who has followed the evidence in *Matthiola* will have seen that the phenomena largely conform to the Mendelian principles. Distinct classes of unconformable phenomena occur, and a few quite irregular cases. These several results cannot be adequately presented in a summary, but in the following tables (Tables XIV and XV) an attempt has been made to give a concise statement of the general results obtained from the various experiments.

Of these twelve aberrant cases (Table XIV), nine we may presume were examples of the "false hybridism" of Millardet. For in these unions, contrary to expectation, a mixed offspring was obtained; but the recessive plants, so produced, bred true—all the offspring obtained from them by two generations of self-fertilisation exhibiting the

Mendelian Cases.

Results obtained when the expectation is all D.			Results obtained when the expectation is all R.		
Reference number of experiment.	Result.	How obtained.	Reference number of experiment.	Result.	How obtained.
1—9, 11—18	1250 D	From various crosses D × R	58*c*—*e*, and others not numbered (p. 79)	1148 R	From self-fertilisation of extracted R's.
19, 20*, 21*, 22*, 24, 25, 27—33, 36, 37, 45, and	796 D	,, ,, ,, R × D	Not numbered (p. 79)	218 R	From self-fertilisation (for one generation) of R's produced in a first cross.
in part 20, 22, 23, 25*, 35, 39, 40, 42, 46	All D (see experiments)				
Not numbered (p. 38)	148 D	,, ,, ,, (R × D) × D	Not numbered (p. 79)	113 R	From self-fertilisation (for two generations) of R's produced in a first cross.
56*b*, 103*b* 104, 105, 106*b*, 107*b*	979 D	From self-fertilisation of extracted (presumably pure) D's	58*a*—*b*	639 R	From crossing extracted R's with R's resulting from self-fertilisation.

Aberrant Cases.

When the expectation is all D.			When the expectation is all R.		
Reference number of experiment.	Result.	How obtained.	Reference number of experiment.	Result.	How obtained.
10	72 R	From [(D × self) × self] × [(R × self) × self]	79, 80	94 D 1R	From crossing extracted R's with R's resulting from self-fertilisation.
(In part) 20, 22, 23, 25*, 35, 39, 40, 42, 46	Mixed D and R (see experiments)	From various crosses R × D			

pectation is a *mixed* offspring.

Cases.

Ratios obtained when the expectation is 3D : 1R.

eference umber of expt.	Ratio. D : R.	How obtained.
68	2·98 : 1	From self-fertilisation of (D × R)
92	3·05 : 1	" " " (R × D)
93	3·83 : 1	" " " "
94	3·00 : 1	" " " "
55	2·92 : 1	" " " [(D × R$_1$) × R$_2$]
84	2·90 : 1	" " " [(R$_1$ × D) × R$_2$]
98	3·13 : 1	" " " "
99	3·22 : 1	" " " "
100*b*	2·51 : 1	" " " "
101	3·16 : 1	" " " "
103*a*	2·65 : 1	" " " [(R × D) × self]
106*a*	4·00 : 1	" " " "
? 102*b*	4·00 : 1	

Average ratio .... **3·18 D : 1R.**
Ratio calculated from the sum of the totals.... **3·22 D : 1R.**

Table XV—*continued.*

Aberrant Cases.

Results obtained when the expectation is 1D : 1R.			Results obtained when the expectation is 3D : 1R.		
Reference number of expt.	**Result.**	**How obtained.**	**Reference number of expt.**	**Result.**	**How obtained.**
87—90	**All D or almost all D**	**From the cross—** $(R_1 \times D) \times R_2$	**100*a* 102*a***	**All D ....**	**From self-fertilisation of—** $[(R_1 \times D) \times R_2]$
60—63	**" " "**	$(D \times R_1) \times R_2$	**51, 54, 56*a*, 66, 67, 81—83, 97**	**Offspring mixed; 1·8D : 1R**	$(D \times R)$ **in 3 cases** $[(R \times D)$ **in 1 case]** $[(D \times R_1) \times R_2]$ **in 4 cases** $[(D \times R) \times$ **self] in 1 case**
75, 76*a*	**" " "**	$[(D \times R_1) \times R_2] \times R_3$			
73*b*	**All D ....**	$[(D \times R_1) \times R_2] \times R_2$	**95, 96, 107*a***	**" " 4·7D : 1R**	$(R \times D)$ **in 2 cases** $[(R \times D) \times$ **self] in 1 case**

(α) That in the two cases in which recessive offspring were altogether absent (Experiments 100*a*, 102*a*), the experiments were of a parallel nature, the dominants in each case arising from the operation $[(R_1 \times D) \times R_2] \times$ self.

(β) That in all such cases (except Experiment 102*a* mentioned in (α)) where the dominant type introduced in the first cross had been one of the annual forms 2, 3*a*, 3*b* (Experiments 51, 54, 66, 67, 81, 82, 83, 97), the proportion of dominants was invariably too low (1·8 D : 1 R). In fact, this aberrant result was universal for all such experiments with the types 2 and 3*a*. Type 3*b*, however, gave a Mendelian result in the two other similar experiments (Experiments 55, 84).

(γ) That in all such cases (except Experiment 100*a* mentioned in (α)), where type 1 had been originally used as a dominant pollen parent, the proportion of dominants was invariably too high (4·73 D : 1 R) (Experiments 95, 96, 107).

[In neither (β) nor (γ) were the operations of precisely the same form as in (α).]

[Note, added March, 1902.

The Aberrant Results obtained from Cross-breds.

colour may give hoary plants, but very rarely, not more than 1 in 1000 perhaps. In view of their evidence the suggestion here made is exceedingly remote, but some reference to this obvious possibility is called for.

In considering the other aberrant group of cases where cross-breds on self-fertilisation gave too high a proportion of recessives with some consistency, the possibility of "false hybridism" on the recessive side should not be forgotten.]

PART II.—POULTRY.

Experiments begun in 1898, *carried out by W. Bateson.*

The two breeds first chosen for experiment were Indian Game and White Leghorn. Experiments were subsequently made with Brown Leghorn, White Dorking, and with a single white Wyandotte hen.

Indian Game are dark birds with yellow legs and pea combs. By a "pea comb" is meant a comb consisting of three fairly regular longitudinal ridges, along each of which are several more or less lumpy tubercles. In both sexes the pea comb is low, that of the cock rising about 1 to 1½ inch from the skull, while that of the hen is only about ¼ to ½ inch high, being in fact rudimentary. In the hen the tubercles may be almost entirely suppressed. Any one who desires to examine such combs can see them any day in a poulterer's shop. From the "single" comb of the Leghorns the pea comb is readily distinguished by the absence of sharp serrations, and by the presence of the lateral ridges (often obliterated in old hens).

In correlation with the low comb, the wattles are also very short, being almost rudimentary in the hen, and only about 1 inch long in the cock. The ear-lobes in both sexes are bright red, like the wattles, but they project very slightly, and though a little full below, they are never pendulous, as in the Leghorn cock.

The plumage of the cock is for the most part black, shot with dark green. The hackles and saddle are broken with a variable amount of dark red, which if noticeable, is considered by the fancier a fault. There is a large brown patch extending across the secondaries.

The plumage of the hen, with the exception of the hackles which are black, is almost entirely composed of black and brown mixed together in almost every feather. The mixture should be in the form called "lacing" (concentric lozenges or rings of colour), which may be either single or double, in the various parts of the body. The quill-feathers are liable to be vermiculated or "mossed" with brown, which is also reckoned a fault.

The details of coloration are complex, but for our purpose need not be described. There is a good deal of variation between strains as regards minor points, but on the whole the breed is a very uniform one.

The down of the newly hatched chicks may be *either* striped dark and light brown, *or* almost entirely without dark stripes, leaving the general colour then a nearly uniform buff. All intermediates may occur, but in individual families there is often a fairly sharp discontinuity in respect of down-coloration, though the birds when grown up may be quite alike.

To the eye, a most striking feature in these birds is the close, "hard" feathering, and upright carriage. It is to the closeness of the feathering that is largely due the appearance of great length of neck and leg, which constitute a prominent character.

The shanks are, in healthy and well-grown birds, of a full deep yellow. In depauperated specimens they are a more dingy yellow.

The bill is either a deep yellow or of a rather dingy horn-colour the comb may increase in size,* taking the form here called "intermediate pea." This comb has the three ridges well marked, but the extent upwards from the skull is in the cock about 2 inches, and in the hen about ¾ of an inch. The intermediate pea in the cock usually falls over to one side. Such a comb would be considered a great disfigurement, but it certainly may occur in good strains. It should be mentioned that the cocks are generally dubbed for exhibition, and consequently the details of comb structure have not been much attended to by fanciers.

For our purpose it is important to notice that the true pea comb and the intermediate pea are hardly if at all distinguishable *in the newly hatched chick.* The distinction does not become evident, especially in females, until sexual maturity approaches. At all ages both are readily distinguishable from the single comb of the Leghorns.

White Leghorns.

The other breed originally chosen was *White Leghorn.* Leghorns as a definite breed are probably at least 50 years old, and at the present time exist in many colours which breed fairly true. One of the most definite is the white breed, which can be absolutely relied on to propagate itself without notable variation.

The comb is a high single comb, with one row of large serrations of varying number, including at least four or five large points distinct from the more or less irregularly divided anterior and posterior lobes. In the cock the comb is erect. Measured from the skull to the tip of the serration next over the eye, the height in an adult is 3—4 inches, varying in actual size both with age, treatment, and condition. In full-grown cocks it projects forwards over the bill and backwards behind the head, the total length being about 5 inches. Fine prize birds in show condition may considerably exceed these measurements.

In the hen the comb is similar though smaller, usually lying over to one side, the correct position in the fanciers' opinion. The size in hens varies a good deal in individuals, and greatly with condition. About 2 inches high and 3 inches long may be given as average dimensions, greatly exceeded in breeding condition in some cases. In correlation with the high comb, the wattles are long, reaching in the cock to a length of some 4½ inches from the angle of the gape. In the hen they are about 2 inches long. The ear-lobes are bluish or yellowish-white and pendulous, increasing in size greatly with age.

The plumage is entirely white, without the least speck of any colour. It is *never* close-fitting as in Indian Game.

Legs and bill are pale yellow. The older females frequently (? in all strains) have large spurs without any decline in fertility. The eggs are white, and produced in great numbers. The hens do not as a rule sit or show any sign of broodiness.

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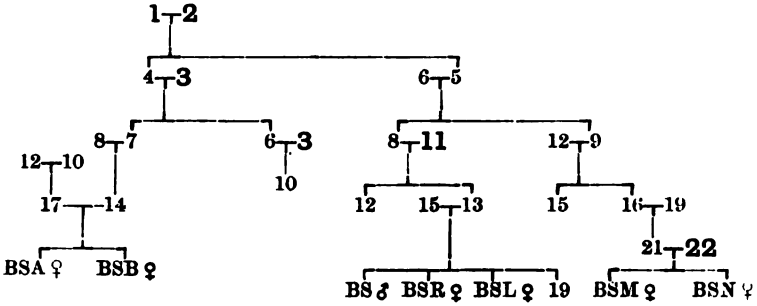
* This has been my experience, but I am not sure if the fact is generally true.

These breeds seemed specially suitable, because they differed from each other in their combs, a structural character. The great apparent difference in proportional length of leg bones, which at first sight seemed capable of measurement, proved on examination to be due almost entirely to the difference in closeness of feathering, the loose feathers of the Leghorns altering the general outline of the body, and making the legs appear shorter by comparison.

A special reason for choosing these two breeds, apart from the general suitability, was that it was possible to obtain birds of each, with a statement as to their immediate pedigree. The stock of Indian Game was supplied by the Rev. E. P. Boys-Smith, vicar of Hordle, Lymington, Hants, who had bred them with care for some years, keeping careful pedigrees. The original White Leghorns came from Mr. E. C. Lister-Kay, of Burley Manor, Ringwood, Hants, and of them also I was given a pedigree, carefully kept though incomplete as regards the individual identity of the hens, several sister hens being run with one cock. Both these gentlemen have supplied many practical hints and information, which have proved most useful.

In the first two years of experiment many troubles occurred, mostly importance may be attached to the details of relationship, I subjoin the pedigree for future reference if required. The extent to which the birds were in-bred may be gathered from the fact that in seven generations only four unrelated birds occur, and the last introduction of fresh blood, or the hen which stands as great-grandmother to one lot and grandmother to the other. The mating generally used was nephew and aunt.

Pedigree of Indian Game Birds received from Mr. Boys-Smith.



In the above pedigree the male is on the left side in each mating. The Clarendon figures show the introduced birds. No doubt all are in some degree related, as the best strains are in few hands.

3. Two hens, full sisters, from Mr. Boys-Smith, sent in 1901, being then in their second breeding season. These are indicated in the pedigree as BSM ♀ and BSN ♀.

4. Cockerel and pullet from the Rev. C. T. Bromwich (Brom. ♂ Brom. ♀). As to the pedigree of these birds, no account was to be had, but it was said that a new male was introduced each year in their immediate pedigree, and the birds bought were almost certainly either full or half brother and sister. As will be seen hereafter, there is doubt as to whether these birds were "pure" in the critical sense.

5. Cockerel of unknown extraction from Ringwood Poultry Farm. (Rd. IG ♂.)

6. Cockerel of unknown pedigree from a Lancashire breeder. (Gr ♂.) The question of true "purity" is also doubtful in this case.

WHITE LEGHORNS.

(*For description see above.*)

1. Cockerel and pullet from Mr. Lister Kay, not in-bred. (LK ♂, LK ♀.)

From a fancier's point of view these birds were not in-bred at all;

for in their pedigree a fresh cockerel, presumably unrelated, was introduced at each successive generation.

2. Cockerel and three pullets hatched from eggs purchased from Mr. Adeane, Babraham Hall (Babr. ♂, Babr. ♀ 42, Babr. ♀ 89, Babr. ♀ 110). These birds were almost certainly not in-bred. They came from a run where many cocks and hens were together, and it is unlikely they were nearly related to each other, though this may have been the case.

3. A pullet from Ringwood Poultry Farm (Rd.L ♀).

BROWN LEGHORNS.

1. A pullet from Mr. Boys-Smith, sent me in 1900 (Br. L^1).
2. A sister to the foregoing sent in 1901 (Br. L^2).
3. A cock sent in 1901, nephew to the foregoing (Br. L ♂).

As regards structural characters, Brown Leghorns closely resemble White Leghorns. In colour of plumage the browns have bright golden-red hackles, red coverts and saddle, with black breast, quills and tails in the cocks, while the hens are of a duller general mottled-brown colour, the hackles being golden yellow, with black central stripe. The breast is a bright chestnut. The deaf ears are white in all Leghorns,* and the bills and shanks are yellow. Eggs white. Hens do not sit. These birds were closely in-bred, as the following pedigree will show :—

Here No. 17 represents Br. L ♂, and No. 16 the two hens, Br. L 1 and 2. The usual mating is again aunt and nephew. (Father on the left in each mating.)

WHITE DORKINGS.

The White Dorking is a pure white bird with white bill and shanks, and a *rose-comb*. The feathers are loose-fitting, and never close-fitting. A rose-comb consists of a great number of papilliform elevations, standing in the same horizontal plane. It is wide in the middle, and contracts to a point or "pike" behind.

It is recognisable at an early age in embryonic life, and can never, from about the 9th day of incubation, or earlier, be confused with a single comb. The distinction between rose and single is, with the rarest exceptions, absolute, and no class of intermediates needs here to be distinguished. The rose comb rises about 1¼ inches from the skull in cocks and rather less in hens which are laying, while in hens out of breeding condition, it scarcely rises from the head more than half an inch.

The wattles are about 1½ inches long in cocks, and about 1 inch long in hens. They are rounded in shape. The deaf ears are red and project but little, never being to any extent pendulous.

The bill and shanks are pale pinkish-white, without pigment. The breed has the extra or fifth toe, characteristic of all Dorkings. The eggs are long, of a faint stone-colour, and the hens go broody.

WHITE DORKINGS.

1. Two pullets received from Mr. O. Cresswell, Morney Cross, Hereford, in 1901 (Cr 121 and Cr 122). In Cr 121 a faint tinge of chestnut appeared on one secondary of each wing after the moult of the second year (see p. 98).

2. A cockerel received at the same time from the same source. (Cr ♂.)

3. A pullet received from Miss Knowles, Conholt Park, Andover, in 1900 (K ♀).

4. A cockerel from the same source (K ♂).

From the two birds K ♀ × K ♂ were raised two pullets, Dork. 262 ♀, Dork. 263 ♀, and one cock, Dork. 258 ♂.

Though I have no pedigree with either of these lots, Mr. Cresswell informs me that his birds are very closely bred, but the details are unknown. The breeding of Miss Knowles' birds is also unknown, though in all probability they are not specially in-bred. These two strains are only remotely related. As will appear, it became practically certain later that one (? both) of the K birds was not pure in respect of *rose comb*. In reply to an inquiry, Miss Knowles was so good as to send information that single combs appear occasionally among her chicks, perhaps 1 in 30, especially when fresh blood is introduced, confirming the view here given. The bearing of these facts will be considered later.

White Wyandotte.

The Wyandotte is another loose-feathered rose-combed breed, but the comb differs from the rose of the Dorking in the fact that the posterior point is directed slightly *downwards* instead of backwards. For our purposes the only other feature of importance is that the deaf ears are round and red, and the bill and shanks yellow. The white variety is not a well-recognised breed, and I know nothing of it beyond the fact that I obtained such a pullet from Ringwood poultry farm. Pedigree unknown.

Before giving the details of specific experiments, it will be useful to state in general terms the results when the various breeds are crossed together. Except in regard to the distribution of pigment in the White Leghorn × Indian Game crosses, reciprocal unions do not differ in result.

Crosses between White Leghorn and Indian Game: General Account.

1. *Comb and Wattles.*—In the cross-bred the comb is almost always

2. *Plumage-colour.*—Of the hundreds of crosses made both ways, all, with one single exception, were white or dingy white* coloured birds, more or less speckled or "ticked" with black. Such ticking is rarely extensive, and is quite irregular and asymmetrical. The ticking may affect parts of feathers, single feathers, or small patches of feathers.

The ticks may be either plain black, or disposed as barrings or chequerings, especially in hackles and saddle-feathers.† The details of these markings have probably some significance, and may perhaps be described on another occasion.

In addition to the dark ticks the cocks not rarely have patches of reddish or brown colour ("pile") on the wing-covers and saddle rarely as developed as in pile breeds. Such brown marks occur occasionally in hens, but are very small. The brown marks may rarely be barred or chequered.

In respect of ground-colour reciprocal crosses give the same result in *male* offspring. All male cross-breds, and *female* cross-breds *from Indian Game mothers*, have the ground-colour *white*, but *female cross-breds from White Leghorn mothers* almost always have the ground-colour more or less dingy brownish-white. The dinginess is sometimes hardly noticeable, but occasionally it is so striking as to make the bird look a dirty cream colour. It is clearly due to a general deposition of a very fine layer of pigment. Such "dingy" hens, though ticked as the others are with pure black, *never have barred or chequered* feathers, so far as our experiences goes.

In the dingy hens the skin of the face is generally somewhat pigmented, though in various degrees, sometimes approaching to the "purple-face" of Brown-breasted Game and other breeds. No dinginess was ever seen in a male bird.

3. *Bill and Shanks.*—These are full yellow, except in "dingy" hens. In the latter there is pigmentation irregular in amount, varying in the case of the shank from slight patches to a general dark olive. The soles of the feet are always yellow. The bills of dingy hens are striped asymmetrically and irregularly with black or brown. The degree of blackness in shanks is correlated with that in bill fairly closely. This pigmentation begins when maturity is approached and cannot be detected in the shanks of young chicks. Of all the female cross-breds with I.G. fathers which reached the age for pigmentation, only one failed to exhibit it either in bill, or shanks, or in both.

4. *Deaf Ears.*—These are usually patchy, partly red and partly

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* The exception was an extraordinary piebald chick which died young. The middle of the back was occupied by a large irregular island of striped colours, like those of a normal Ind. Game chick. This patch extended asymmetrically on to the proximal parts of the limbs.

† These barred feathers were noticed by Darwin in poultry crosses ('Animals and Plants,' *Ed. 2, vol. 2,* p. 255), and regarded by him as reversionary.

white, showing a mixture of the parental characters both in colour and shape.

5. *Carriage.*—The attitude of the crosses is much more like I.G. than Leghorn. Though not so "reachy" as a fine Indian Game, they never have the general appearance of Leghorn. This appearance is doubtless due to the fact that the hard, close feathering of the I.G. is always in some degree dominant.

6. *Eggs.*—The shells are usually of intermediate tint. Eggs brown enough or white enough to pass for pure I.G. or pure L. are decidedly rare.

7. *Broodiness.*—Many of the cross-breds became broody and made fair mothers. Some were very uncertain, but perhaps not more so than many hens of sitting breeds. Some did not go broody at all.

In regard to colour of eggs, fertility, and broodiness, for want of opportunities no extensive observations were made.] Moreover, for any adequate study of most of the other points enumerated, it is necessary that the chickens should be reared to maturity. To do this with large numbers is quite beyond our resources. Fortunately, however, the comb-structure can be determined with certainty in very young chicks at about the 14th day of incubation. By the use of this fact the range of available statistics has been greatly increased. For by opening the eggs on the 15th day, the same incubator can be recognise cross-bred pea combs (DR) from pure peas (DD), at all events in cocks from the first, but the hens are much more difficult.

First Crosses between Brown Leghorn and Indian Game: General Account.

Only a small number of birds of this cross have been reared to maturity, namely, five cocks and eight hens, the offspring of one Brown Leghorn hen and two Indian Game cocks; and one cock bred reciprocally. These cross-bred birds had dark plumage, which may be sufficiently described as compounded of the parental colours and forms: bill, horn colour; legs, yellow; ear-lobes, white tinged with yellow like Leghorn, feathering and carriage intermediate, but nearer to the Game; comb and wattles as in the crosses between Indian Game and White Leghorn.

White Leghorn with White Rose-combed Dorking.

The offspring is always white. The bill and shanks are white like the Dorking, or rarely with a faint tinge of yellow.

In point of carriage and disposition of feathering the two parent forms are so nearly alike, that for our purposes these characters cannot be used as a criterion of inheritance. Deaf ears usually red, or red patched with white, in shape intermediate.

The comb is *either* single as in Leghorn, differing only in reduced height, *or* rose as in the Dorking; but often raised from the head rather more than in the normal form. Wattles follow the comb closely. Some have and some have not the extra toe;* details given later.

As regards characters appearing in later life, such as egg-colour, breeding habits, spurring of hens, &c., the numbers reared to adult life were insufficient to warrant a statement.

In the great majority of cases the extra toe is *either* palpably present as a distinct digit *or* absent. When present the two hind toes may consist, as in the normal Dorking, of a short toe like the hallux of a 4-toed bird, with a long many-jointed digit proximal to it pointing upwards. The two, however, may often be *both* short, pointing downwards, never both long. This condition ranges through many stages of bigemination down to mere bifidity of the nail. A form very rarely seen is an *elongation* of the hallux without any extra toe being present.† In such a hallux there is increase in the number of phalangeal joints. This of course corresponds to the three-jointed pollex in man. More or less complete webbing is common in cross-breds.

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* This foot-character can be sufficiently made out in chicks after about 12 days of incubation.

† [A chick has lately occurred with *such a "long" hallux bigeminus*—probably a hitherto unrecorded form.] March, 1902.

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Brown Leghorn c

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statistics can be obtained in regard to them than as to the other characters. In what follows, therefore, primary regard is had to these characters, though some important considerations arising out of the other characters must also be considered later.

Indication of the Mendelian Laws.—Description of the detailed results may be greatly facilitated if we anticipate the recital of experiments, and state at once that in a considerable group of the crossings the operation of the Mendelian laws can be perceived. This group will be considered first, and for convenience will be referred to as the Mendelian group. Many complications must be dealt with, but so far nothing conflicts with the main result.

The cases grouped as Mendelian all show clear dominance. By speaking of the remainder as non-Mendelian it is not suggested that the heredity is not in accordance with the Mendelian principles; but merely, that as there is a condition more complex than that of simple dominance, the application of the Mendelian principles cannot be clearly followed out.

Using the Indian Game birds of

Sections 1 and 2, and their direct descendants ...	p. 90
Section 3 ...	p. 91
The hen of Section 4 ...	p. 91
The cock of Section 5 ...	p. 91
The Dorkings of Sections 1 and 2 ...	pp. 91 and 92
and the Wyandotte hen ...	p. 94

with any of the Leghorns, male or female, the facts show that

pea comb,
rose comb,
extra toe*

are *dominant* characters, and that

single comb,
the normal foot

are *recessive* characters.

We are thus provided with a terminology and a *rationale* of phenomena, which when we come to the more complex results will be found a valuable guide.

Purity of Strains.—A word must now be said as to the purity of the strains. When the Mendelian principles are appreciated, purity acquires a new and precise meaning. This we shall subsequently discuss (see p. 134). For the present it will suffice to say that in regard to the various Leghorns, we have no evidence of a bird, the offspring of two Leghorns, having a comb other than single or a foot other than four-twenty to fifty.

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* The dominance of this character, as will be seen, is not absolute, and a cross-bred without any trace of extra toe may be DR in respect of that character.

MEN

Crosses between

Details of Exp

The following lists give the
comb obtained by crossing th
horns. The breeding of the I
experiments.

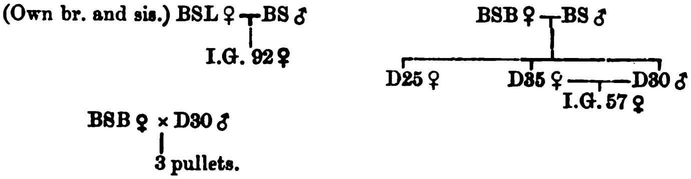
By "1 year," "2 years," &
that it was in its first or secc
sequent experiments are numl

†1. I.G. BSA ♀ (1 yea
2. „ BSL ♀ (2 year
3. „ BSR ♀ (2 year
4. „ BSB ♀ (2 year

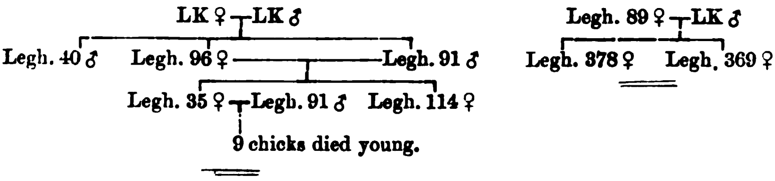
From BSB ♀ (one year) × B
thoroughbred Indian Games—
Leghorns, gave the following :

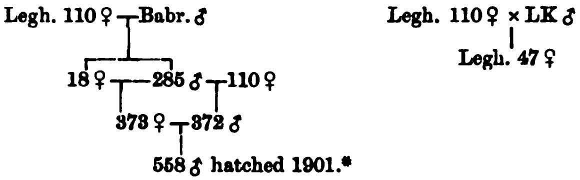
5. Babr. 110 ♀ (1 year) ×
6. Babr. 89 ♀ (1 year) ×
7. BrL1 ♀ (1 year) ×
8. Unknown white

From the original Indian Game of the Mendelian sections, thoroughbreds were raised as follows:



From the original White Leghorns, thoroughbreds were raised as follows:





Using these birds, the following results were obtained:

			P.c.	S.c.
9.	I.G.	35 ♀ (1 year) × Babr. ♂ (1 year)................	7	0
10.	„	25 ♀ (1 year) × „ (1 year)...............	4	0
11.	„	57 ♀ (1 year) × Legh. 285 (1 year)...........	6	0
12.	„	3 pullets (BSB ♀ × D30 ♂) × Legh. 355 ♂ (1 year)	12	0

All pullets used in Experiments 9—12, were late-hatched the year before, and consequently laid few eggs, apart from any question of effects of in-breeding.

			P.c.	S.c.
13.	I.G.	92 ♀ (1 year) × LK ♂ (2 years)................	18	1
14.	„	„ (2 years) × 91 (LK × LK)..................	23	0

From matings 1—14, in some of which the Leghorn was father, in others mother, were produced pea comb 182, single comb 1, so that, with one single exception there was complete dominance of the pea comb.

To these cases of complete dominance we may add the following, in which it must be noticed that Indian Game blood from a new source is introduced. From Indian Game, BSR ♀ × Indian Game Brom. ♂, a

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* The numbers in this pedigree are those by which the birds are spoken of.

16. ,, ,, 89 ♀ (2 yea
17. Brown Legh. 1 ♀ (2 yea
18. Legh. 96 ♀ (1 year) × I.G
19. ,, 35 ♀ (2 years) ×
19*a*. I.G. 120 ♀ (1 year) × Leg

In all, therefore, these two
ful s.c.

The other Mendelian India

20. I. G. Brom. ♀ (1 year) ×
21. Legh. Babr. 89 ♀ (1 year)
22. I.G. BSN ♀ (2 years) × L
22*a*. ,, ,, (2 years) × Br
23. ,, BSM ♀ (2 years) × L

From these 25 matings, th
and 1 doubtful.

Crosses between Le

Details of Experiments in regar

24. Cr ♀ 121 (1 year) × Legh. 285
25. ,, (2 years) × Brown
(2 years)
26. Cr ♀ 122 (1 year) × Legh. Bab
27. Legh. 18 ♀ (1 year) × Cr ♂ (1

Taken together there were—

R.c.	Interm.	S.c.	E.t. both sides.	E.t. one side.	No e.t.
123	1	1	100	5	2

The dominance of the rose comb is therefore almost complete, while that of the extra toe, though very strong, is not quite so complete. Experiment 25 proves that the dominance of the extra toe was distinctly imperfect as against the normal foot of the Brown Leghorn ♂. These results are referred to on p. 113.

White Wyandotte and White Leghorns.

		R.c.	S.c.
30.	Wy. ♀ (1 year) × Legh. Babr. ♂ (1 year)……	17	0
31.	„ (2 years) × „ LK ♂ (3 years)…….	5	0
32.	„ (1 year) × „ 40 ♂ (1 year)………	3	0
33.	„ (2 years) × „ 91 ♂ (1 year)………	20	0

In all, therefore, this hen produced 45 rose combed chicks by four cocks and no single comb.* All the chicks reared were white, or white with a few ticks of black, barred in several. The general shape agreed nearly with the Wyandotte: legs and bill yellow, ear-lobes red patched with white.

Experiments 1—33, therefore, show that the pea comb, rose comb, and extra toe are in certain strains dominant almost without exception. A distinct departure from this regular dominance as regards toe is seen in Experiment 25 (see p. 113).

First Crosses bred together.

34. From Experiment 17, Brown Leghorn 1 ♀, by Indian Game 59 ♂, 4 pullets and 1 cockerel were taken, and their eggs incubated through the whole breeding season of 1901.

They gave 166 pea combs and 59 single-combs, or 2·8 : 1. All the chicks were more or less dark on hatching. Those that were lightest were buff but had dark quills showing, so that there is no doubt they would all have proved dark birds.

35. The cock (dark; pea comb) used in Experiment 34 was put with 5 hens (light; pea comb), viz., 3 that had been used in Experiment 36, 1 bred in Experiment 14, and 1 bred in Experiment 5. This was at the end of the season, 1901, when most hens had stopped laying. It was not known which hens laid the eggs, but they gave collectively 19 p.c. and 6 s.c., or 3·1 : 1.

The colour-result was altogether *contrary* to simple expectation on the hypothesis that the colorations of Brown Leghorn and Indian Game can be treated as both alike "dark." The chicks were nineteen dark and six light, whereas equality would be expected on the above hypothesis. [See note, p. 141.]

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* Mr. Grosvenor Berry informs us that in crosses he has made with Golden Wyandotte, the rose comb was always dominant, without exception, over single comb.

36. From Experiment 17 (Leghorn Babr. 42 ♀ × Indian Game 59 ♂) four pullets and one cockerel were similarly bred together for the season, 1901, with the result 147 pea comb, and 45 single comb, or 3·27 : 1.

In this case the grandparents were of strikingly different colours, white and dark respectively, and as has been said the first crosses in these matings are light birds. Taken broadly, therefore, *light* is here dominant, and dark is recessive, and, as a matter of fact, the offspring of Experiment 36 consisted of 149 light, and 41 dark, or 3·6 : 1, a proportion not greatly removed from 3 : 1.

Grouped according to both characters, comb and colour, the offspring were—

Light with pea comb	111
,, single comb	37
Dark with pea comb	34
,, single comb..................	8

If the numbers followed strictly Mendel's law, they should be a 9 : 3 : 3 : 1.

no extra toe. Here also the combs depart widely from 3 : 1, being 5 : 1 (*cf.* Experiment 37), while the toes being 78 : 29, agree precisely, being 3·0 : 1. (As to these numbers, see note, p. 140.)

Grouped according to both characters, they were—

R.c. with e.t.	64
R.c. without e.t.	21
S.c. with e.t.	12
S.c. without e.t.	5

The nearest Mendelian series of numbers is 63 : 21 : 21 : 7.

In this experiment six *dark* chicks (not reared) occurred, though both birds in Experiment 24 were light (see p. 98).

39. At the end of March the cockerel used in Experiment 38 died of tuberculosis, and the cockerel used in Experiment 37 was put to the hens of Experiment 38. On reference to Experiments 24 and 29, it will be seen that the cock was in composition the reciprocal of the hens. As the results of the two experiments were similar, the hens were put to the new cock without an interval, and consequently a few of the chicks here reckoned were doubtless by the first father. At the end of the season also the hens of Experiment 37 were added to those of Experiment 38, and laid about 10 of the chicks here reckoned.

The results were 118 rose combs, 35 single combs; 131 with both extra toes, 28 with no extra toes. The combs are therefore in the ratio 3·3 : 1 and the feet 4·7 : 1.

Grouped according to both characters, there were—

R.c. with e.t.	94
R.c. without e.t.	23
S.c. with e.t.	32
S.c. without e.t.	3

Here there is a considerable deficiency of no e.t. birds. The nearest Mendelian series is 90 : 30 : 30 : 10.

No dark chicks occurred here.

Experiments 37, 38 and 39 together give—

R.c., 273; s.c., 65; or 4·2 : 1
E.t., 272; no e.t., 79; or 3·4 : 1

Grouped according to both characters—

R.c. with e.t.	208
R.c. without e.t.	63
S.c. with e.t.	54
S.c. without e.t.	11
	336

As was seen in Experiments 37 and 38, the recessive *combs* were deficient, while in Experiment 39 the greater deficiency was in the recessive *feet*. These two deficiencies in this final table partly cancel each other, but as the whole number in Experiment 39 was far greater than in the other two, the table is overweighted by the peculiarity of that family.

In the chief departures from 3 : 1, the near approaches to 5 : 1 should be noticed, viz. :—

In Experiment 37 the combs are ...... 5·4 : 1
„ 38 „ „ ...... 5·0 : 1
„ 39 the feet are .... 4·7 : 1

Whether this is a mere coincidence without further significance cannot yet be said.

Wyandotte-Leghorn First Crosses bred together.

40. Four pullets and one cockerel from Experiment 33 bred together until end of July, 1901, gave 196 rose combs and 79 singles, which is approximately only 2·5 : 1. Here the *recessives* for the first time considerably exceed the expected proportion of 1 : 3.

It is a remarkable fact that in this mating a number of *dark* chicks

Dominant First Crosses bred with Pure Recessive.

41. The four pullets used in Experiment 40 after their resting-season were for the remainder of 1901 put with a pure Leghorn cockerel Leg. 369 ♂. They gave 46 r.c. and 50 s.c., or almost equality, which the law demands. All chicks of course light in colour. This experiment was very kindly carried out for me by Mr. R. Assheton, Lecturer on Biology to Guy's Hospital.

42. A similar experiment was made with three dominant first crosses raised respectively in Experiments 5, 15, and 16. Put with a pure Leghorn (355 ♂), for the latter weeks of 1901 they gave—

P.c.	S.c.	Light down.	Dark down.
28	30	57	0

Here again the approach to equality is very close. There are of course no dark coloured chicks, as only one grandparent was coloured.

Nevertheless, that the peculiarity of the male if permanent is not of itself enough to explain the aberrations, is shown by a comparison of Experiments 37 with 38 and 39. In Experiment 37 the same male was used as in Experiment 39, the females being the same in Experiments 38 and 39. But while the feet are practically 3D : 1R in Experiments 37 and 38, they are 4·7 : 1 in Experiment 39. Conversely, while the combs are 3·3 : 1 in Experiment 39, they are about 5 : 1 in the other two experiments. There is at present no obvious explanation of these results, and we can only accept the conclusion that not only different individuals similarly bred may give different proportions, but that these proportions may change also at different times (*cf.*, p. 121).

TOTALS OF MENDELIAN FIRST CROSSES BRED TOGETHER.

Experiments 34—39, added together, give the following totals. Though not strictly legitimate, such promiscuous addition is rendered less objectionable for the reason that in the case of each character which departs considerably from 3 : 1, there are grounds for regarding such a departure as an oscillation and not a departure peculiar to the character.

Pea comb and single comb—

	P.c.	S.c.
Experiment 34............	166	59
„ 35...........	19	6
„ 36...........	147	45
	332	110 or 3 : 1

Experi

Dominant combs t
Extra toe and no e

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Plumage.—The case
is very doubtful if the
others at all. As was
white, birds. We may
“dark.” Moreover, as
young chicks, they can
But from those that w
“dark” class consists of
of which are recurrent a
At the present time st

ments 36 and 76—80) gave some blacks, some cuckoos, and some nearly approaching Indian Game.

In each case the "light" classes consist of some pure white birds, many white birds flecked with black or red, such as occur in the first crosses; and in the Br. Legh.-Dorking crosses, some white birds with chestnut breasts ("pile").

Until the experiment is completed, no statement can be made, but the opinion may be hazarded that at least some of these colour forms, if corresponding cocks and hens were mated, would breed true from their first appearance (see p. 144).

The Experiments 76—81, from which the colour results are now under discussion, were made with families which were non-Mendelian as regards structural characters, but as regards colour these families were Mendelian in the sense that "light" was dominant against "dark."

Remembering the *caveat* expressed above, taking all cases in which the first crosses between dark and white breeds have been bred together, we have the following results :—

	Light down.	Dark down.
Experiment 36...........	149	41
„ 76...........	50	10
„ 77...........	49	16
„ 78...........	56	24
„ 79...........	106	40
„ 80...........	8	1
„ 81...........	131	44
	549	176 or 3·1 : 1

GRAND TOTALS.

Adding all dominants and all recessives, we have—

	Dom.	Rec.
Pea comb and single	332	110
Dorking rose comb and single ...	273	65
Wyandotte rose comb and single	196	79
Extra toe and no extra toe	272	79
Light down and dark down	549	176
	1622	509 or 3·18 : 1

Though this total agrees closely with the expected 3 : 1, it must not be forgotten that several of the observations which contribute to the result show considerable departures from it, and we may suspect that some of these are not merely fortuitous accidents.

For *the present* also, we do not know that the extracted recessives are pure, or that the dominants consist of pure and crossed, as they may be expected to do, in the proportions of 1 : 2. But taken generally, there is so close an agreement with expectation that these propositions on which the expectation is based, can scarcely be other than at least a fair approximation to the truth.

Colour of Legs.

The colour of legs and bills cannot be safely determined till maturity, and consequently the numbers are too small to warrant a statement of statistical proportions. It may nevertheless be stated that *qualitatively* the offspring of first crosses between White Dorking and the Leghorns agree with the Mendelian expectation. As stated above, such first crosses have whitish legs, but the offspring of the first crosses bred *inter se* have *either* white (including whitish) legs *or* yellow legs.

General Result.

Taking together the structural characters of comb and foot and the colour of legs it appears that each is transmitted independently. As a result, therefore, the several characters may be combined in individuals bred from first crosses *inter se* in any combination, producing, for example, birds with single comb, extra toes and white legs; rose comb, no extra toe and yellow legs, &c., thus fulfilling the

On the contrary, the cock in 4 (p. 91) Brom. ♂ and the cock in 6 (p. 91), Gr. ♂, gave mixed offspring with Leghorn hens, as follows:—

		P.c.	S.c.
43.	Legh. LK ♀ (1 year) × I.G. Brom. ♂ (1 year)....	7	18
44.	„ Babr. 42 ♀ (1 year) × I.G. Brom. ♂ (2 years)	10	10
45.	„ Rd. ♀ (1 year) × I.G. Brom. ♂ (2 years) ...	7	5
46.	„ 47 ♀ (1 year) × I.G. Gr. ♂ (1 year)........	14	21
47.	„ 47 ♀ (2 years) and 89 ♀ (3 years) } × I.G. Gr. ♀ (2 years).	15	8
	Of these 5 p.c. and 3 s.c. were laid by 89 ♀ alone after removal of 47 ♀.		
48.	Legh. ♀ (breeding unknown) × I.G. Gr. ♂ (1 year)	8	4
	This hen was used in Experiment 8 (*q.v.*).		

Each mating therefore in which these cocks were used, resulted in a mixture of pea and single, though the proportions vary greatly.

A family (see table, p. 112) of five Indian Game birds were raised from the hen BLR ♀ and Brom. ♂. Of these, three were females, viz., I.G. 17 ♀, I.G. 90 ♀, and I.G. 94 ♀, and two were males, I.G. 12 ♂ and I.G. 15 ♂. Bred with Leghorns these five birds gave mixtures as follows:—

		P.c.	S.c.
49.	I.G. 17 ♀ (1 year) × Babr. ♂ (1 year)...........	20	16
50.	„ (2 years) × Legh. 285 ♂ (1 year)	16	14
51.	„ (3 years) × Legh. 673 ♂ (1 year)	12	11
52.	I.G. 94 ♀ (1 year) × Legh. 40 ♂ (1 year)	2	8
53.	„ (2 years) × Legh. LK ♂ (2 years)......	15	10
54.	„ (2 years) × Legh. 91 ♂ (1 year)........	9	16
55.	„ (3 years) × Legh. 91 ♂ (2 years).......	4	1
56.	I.G. 90 ♀ (2 years) × „ (1 year)	6	12
57.	„ (3 years) × „ (2 years)	6	2
58.	„ (2 years) × Legh. 285 ♂ (1 year)	5	3
59.	„ (3 years) × Brown Legh. ♂ (2 years)...	13	11
60.	Legh. Babr. 89 ♀ (2 years) × I.G. 15 ♂ (2 years)..	8	5
61.	Legh. Babr. 110 ♀ (1 year) × „ (1 year) ..	12	12
62.	Brown Legh. ♀ Br. L. 1 (1 year) × I.G. 12 ♂ (1 year)	4	8

It is seen, therefore, that each of three hens and two cocks, raised as above, gave mixtures when bred with various Leghorns, and again the proportions of such mixtures vary greatly. Two of these I.G birds, out of BSR ♀ × Brom. ♂, viz., I.G. 90 ♀ and I.G. 12 ♂, were selected, and from them were raised a family of which two only survived. This family consisted of *six chicks with single combs* (see p. 112) and six with pea combs, among which were the two survivors, I.G. 59 ♂ and 120 ♀. The supposed "pure" I.G. birds, therefore, themselves gave a mixture; but the only two survivors, themselves pea-combed, gave exclusively pea-combed offspring, with one doubtful exception only (see Experiments 15—19A).

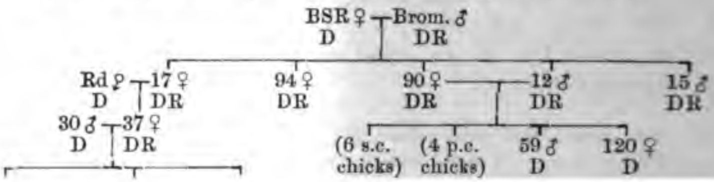
From I.G. 17 ♀ × I.G. Rd. ♂ was produced a family of thirteen hicks, all pea comb. Of these, one, I.G. 37 ♀, was crossed with a eghorn ♂ as follows :—

		P.c.	S.c.
63.	I.G. 37 ♀ (2 years) × Legh. 373 ♂ (1 year)........	28	29

Previous to the foregoing crosses, I.G. 37 ♀ was bred with I.G. 30 ♂, roducing a series of chicks all pea combed, of which two pullets, .G. 416 ♀ and 417 ♀, and one cockerel I.G. 419 ♂, have been rossed with Leghorns as follows :—

		P.c.	S.c.
64.	Legh. 47 ♀ (2 years) × I.G. 419 ♂ (1 year).......	4	4
65.	" 96 ♀ (2 years) × " (1 year).......	11	14
66.	" 114 ♀ (1 year) × " (1 year).......	2	5
67.	" 372 ♀ (1 year) × " (1 year).......	7	10
68.	I.G. 416 ♀ (1 year) × Legh. 91 ♂ (2 years)......	3	8
69.	" 417 ♀ (1 year) × " 369 ♂ (1 year)......	6	4

Table of Relationship of I.G. Birds which gave Mixtures.



Of the two birds, ♂ and ♀, from Miss Knowles, the cock only was tried with Leghorns. In each case he gave some recessives, both as regards comb and toe, as follows :—

70. Legh. Babr. 42 ♀ (2 years) × Dork. K. ♂ (1 year) gave in 1900—

R.c.	S.c.	Both e.ts.	1 e.t.	No e.t.
30	18	46	1	1

71. Legh. Babr. 42 ♀ (3 years)
with
Legh. Babr. 110 ♀ (3 years) } × Dork. K. ♂ (2 years) gave in 1901—

R.c.	S.c.	Both e.ts.	1 e.t.	No e.t.
31	21	64	1	2

72. Legh. Babr. 42 ♀ (3 years) alone × Dork. K. ♂ (2 years) gave in 1901—

R.c.	S.c.	Both e.ts.	1 e.t.	No e.t.
29	19	36	1	0

73. Legh. Babr. 110 ♀ (3 years) alone × Dork. K. ♂ (2 years) gave in 1901—

R.c.	S.c.	Both e.ts.
8	7	15

74. Brown Legh. Br. L. 1 ♀ (2 years) × Dork. K. ♂ (1 year) gave in 1900—

R.c.	S.c.	Both e.ts.	1 e.t.	No e.t.
35	36	59	7	5

75. Br. L. 1 and Br. L. 2 together (3 years) × Dork. K. ♂ (2 years) gave in 1901—

R.c.	S.c.	Both e.ts.	1 e.t.	No e.t.
57	68	93	17	16

Between these two hens the last results cannot be allotted with certainty, but it was known that Br. L. 1 ♀ contributed to these totals at the end of the season after removal of Br. L. 2 ♀.

R.c.	S.c.	Both e.ts.	1 e.t.	No e.t.
25	28	46	3	5

25. In regard to *comb* this experiment has been given in the Mendelian group, for the r.c. showed full dominance so far as the numbers went. As regards foot-character the case belongs here.

Cr. ♀ 121 (2 years) × Brown Legh. ♂ (2 years) gave—

R.c.	S.c.	Both e.ts.	1 e.t.	No e.t.
22	0	16	4	2

It is to be remembered that the hen belonged to the strain of White *Dorkings which, with* other Leghorns, showed dominance of extra toe.

1

of the previous year, and s
hatched."

77. Legh. 378 ♀ (1 year) ×

R.c.	S.c
19	36

78. Dork. 262 ♀ (1 year) ×

R.c.
1

79. Dork. 263 ♀ (1 year) ×

R.c.
4

Matings with Memb

A. *From In*

Three first-cross pullets 111
149 ♀, a single-comb bird, we
(all raised in Experiment 58),
birds were all white, ticked.

80. Cross-bred 111 ♀ p.c. (
gave—

P.c.	S.

83. Three s.c. pullets, first crosses, cross-bred, ♀'s 44, 71, 117 (1 year), raised in Experiment 50, were bred for most of the season with a single comb cross-bred 97 ♂ (1 year) raised in Experiment 54. All four were white ticked birds. They gave—

P.c.	S.c.	Lt. down.	Dk. down.
0	149	106	40

These four birds with recessive combs therefore produced no dominant comb at all.

84. A pea comb cross-bred 77 ♀ (1 year) raised in Experiment 54 with the same cockerel as used in Experiment 83, for a few days only gave—

P.c.	S.c.	Lt. down.	Dk. down.
6	3	8	1

B. *From White Dorkings and Leghorns.*

85. Two r.c., e.t. pullets, cross-bred, 249 ♀ and 278 ♀, raised in Experiment 74 (out of Brown Leghorn 1 ♀ × Dorking K ♂), bred for the season 1901 with own r.c., e.t., brother gave—

R.c.	S.c.	Both e.ts.	1 e.t.	No e.t.	Lt. down.	Dk. down.
131	53	135	8	49	131	44

86. A pullet, s.c., e.t., cross-bred, 317 ♀, raised in Experiment 70, was bred till end of May, 1901, with cockerel s.c., *no* e.t., cross-bred, 305 , raised in Experiment 74, gave—

R.c.	S.c.	Both e.ts.	1 e.t.	No e.t.	Lt. down.	Dk. down.
0	60	45	1	14	60	0

In this experiment again, the birds with recessive combs have been unable to produce any dominant comb. On the other hand, the fact that the cockerel was without extra toes does not seem to have had any effect on the proportion of e.t. to no e.t., which are approximately 3 : 1, as would have been expected if the parents had been dominant first crosses from a Mendelian family. These birds had only one coloured grandparent, and are therefore unable to produce any dark-coloured offspring, in conformity to the expectation.

87. A pullet, own sister to those used in Experiment 86 (raised in Experiment 70), but *rose* combed, with extra toes, by the same ♂ 305, s.c., *no* e.t., gave—

R.c.	S.c.	Both e.ts.	No e.t.	Lt. down.	Dk. down.
8	7	10	5	15	0

88. The same cockerel, cross-bred 305 ♂, used in Experiments 86 and 87, was put for the latter weeks of the season 1901 with six

pure Leghorn hens of various ages and breeding. They collectively gave—

R.c.	S.c.	Both e.ts.	1 e.t.	No e.t.
0	152	77	1	78

138 are recorded as light chicks, and there were none dark.

Here again the cockerel, *without extra toes*, gives the same result as if he had been a Mendelian dominant first cross! The absence of dark-coloured chicks is, of course, to be expected, as only one grandparent was coloured.

Discussion of Non-Mendelian Group.

It has been seen that in the crosses both of Indian Game with Leghorns, and of Dorkings with Leghorns, the results divide themselves into two groups. In the first group the first crosses, almost without exception, exhibit the dominant characters, and the subsequent progeny produced by breeding these first crosses *inter se* obey Mendel's Law fairly well. There are departures from the law which may possibly indicate special disturbing causes, but the general applicability of the principle is unmistakable.

The second or non-Mendelian group of cases is much more obscure.

for finally answering this question in the case of the *combs* is not before us. It was not until the bearing of the Mendelian discovery was fully realised, and its applicability to these phenomena shown, that the absolute necessity for a decision on this point became evident. Moreover, until at the very least three successive generations have been bred the point cannot be quite decided. We may then usefully at this stage consider the facts briefly in the light of each of these two hypotheses, and point out how far either of these suppositions is consistent with observation.

First Hypothesis: that every Recessive is, in composition, RR.

According to this view the two original Brom. birds must be regarded as of different composition, the ♀ giving × Leghorn LK ♂ (Experiment 20), 18 p.c. and 0 s.c. is probably to be considered as pure dominant, DD. The ♂, however, gave by three different Leghorn hens in

	P.c.	S.c.
Experiment 43........	7	18
„ 44........	10	10
„ 45........	7	5

He would therefore be regarded as a dominant cross, DR. We note that in Experiment 43 the numbers 7 : 18 are far from the expected 1 : 1, being 1 : 2·5. To this point reference will again be made.

The same ♂, whose possible "impurity" was then unsuspected, bred (see table, p. 112) with I.G. BSR ♀ of undoubted "purity," gave five birds, of course all p.c. We expect, then, that of these half were DD and half DR. But *all* these five I.G.'s, when crossed with Leghorns, gave mixtures, as told in Experiments 45—58, the proportion of p.c. to s.c. being again very various, though, as a matter of fact, the totals, 122 p.c., 113 s.c., approach the expected equality. Five is a small number for such a test, still it is noticeable that *none* of them appeared to be DD, while on this hypothesis *half* should have been so.

Of these five birds two were bred together, and, as told on p. 112, there was at once a mixture of peas and singles, 6 p.c., 6 s.c., the hypothesis expecting 3 p.c. : 1 s.c. Of these birds, unfortunately, two only, both p.cs., survived to be tested. *Both were pure dominants DD*, though DD : DR should here be 1 : 2.

One of the five birds (I.G. ♀ 17) here assumed to be DR. with three Leghorn ♂'s gave 48 p.c. and 41 s.c. (Experiments 49—51) closely approaching the expected equality.

This hen was bred with I.G. Rd. ♂. As to the latter the evidence is that he gave (Experiment 21) with a Leghorn ♀ 29 p.c. and 1 s.c., which is a close approximation to purity. With I. G. ♀ 17 he gave a family, all p.cs., of which one alone was tested, I. G. ♀ 37, proving (Experiment 63) to be, on the hypothesis, DR, and giving × Leghorns

373 ♂, 28 p.c. 29 s.c., or the expected 1 : 1. The same hen × I.G. 30 ♂, of established purity (Experiments 5—8), gave a family of Indian Games, all p.c. of course. Three of these were tested (Experiments 64—69), giving in each case a mixture, the proportions again varying.

In addition, therefore, to the five birds reared out of BSR ♀ × Brom. ♀, we have one bird out of I.G. 17 ♀ × Rd. ♂, and three birds out of I.G. 37 ♀ × I.G. 30 ♂, nine birds in all, which gave mixtures on crossing with Leghorns, and *none* which did not give mixtures, though the hypothesis expects that *half* of them would give such mixtures, and that *half* would not. This discrepancy, though far from amounting to proof, nevertheless gives a decided suggestion that the hypothesis we are considering is insufficient to thoroughly explain the phenomena. It must be noticed, too, that all the nine supposed DR birds were *out-bred*.

Taking now the Dorking cases, it is clear that as regards comb the Cresswell strain comes very near "purity," giving, on crossing with Leghorns (Experiments 24—29), 123 r.c., 1 s.c., 1 intermediate.

As regards extra toe, we have already called attention to the fact that as Experiment 25 shows, the evidence is not quite so absolute.

The Knowles strain was represented by two birds, K ♂ and K ♀. The male gave on crossing with Leghorns mixtures both as *extra toe*, nevertheless when bred with pure Leghorn hens gave as many chicks with extra toes as without them (Experiment 88). As regards foot, therefore, this cross-bred recessive was not only not pure, but behaved exactly like a cross-bred dominant. Such a fact must be a complete refutation of the hypothesis of "impurity" as a sufficient explanation of the occurrence of recessive foot in the first crosses.

It also suggests very strongly that in cases where neither character has complete dominance, the first cross may produce equal numbers of gametes transmitting each character, just as in strictly Mendelian cases, a point on which more evidence is greatly needed. In such a case selection among the first crosses may effect nothing.

Second Hypothesis: that Recessives produced on the First Cross may be of the Composition DR, the usually dominant Character having Receded.

That this hypothesis is a true one in the case of the extra toe is certain, as stated in the last paragraph. If, then, a character, usually recessive, may thus dominate in the first cross, it may become necessary to regard as crosses some of the recessives produced by cross-bred dominants bred *inter se*, and very serious complexity may thus attend the unravelling of breeding results, though the transmission of character may occur in the strictly Mendelian method by pure gametes.

Seeing that this may certainly be so in the case of feet, we have to consider how the facts stand in regard to comb.

A preliminary consideration which goes against the view that both the Brom. and K. birds were merely impure, in the ordinary sense of being sensibly mongrelised with some other breed, is the fact that none of the birds bred thus far show any unexpected character except in regard to comb. In other characters, notably the brown colour, a character especially complex and likely to show such impurity—there is no noticeable departure from the respective breeds.*

On the hypothesis now before us the question arises, Why is it that some birds should have given mixtures on crossing while others did not? Also, Why do some birds on crossing give mixtures in such different proportions? Until the facts were examined in the light of Mendel's discovery, it was proposed to answer these questions by reference to the suggested effect of *in-breeding*. The B.S. strain had been long in-bred, and the fact that on crossing they gave no mixture was attributed to this cause. On *out-breeding* this strain with Brom. ♂ (see table, p. 112), birds were produced which gave *mixtures* on crossing. On breeding two of these out-bred birds together, brother and sister, two birds, I.G. ♂ 59 and ♀ 120, were produced which were *wholly dominant*, again, as was supposed on account of the close breeding.

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* *Beyond the occasional appearance of white feathers in the wing quills.*

This is again noticeable i
♂ K. Bred with two in-b
seasons (Experiments 74 and
numbers being in all 92 r.c.
White Leghorns, not in-bred,
excess being on the r.c. side (

As regards foot, the eviden
Leghorns scarcely producing
in-bred Brown Leghorns pr
the same kind was seen in
Leghorn was used.

Thus after the first three
males, Leghorns 40 ♂ and 91
distinct excess of singles over
Leghorn 1 ♀ gave a greater
hens did with the same co
dominance of the in-bred
Dorkings seemed to show tha

Reference, however, to the p
Brom. ♂ (Experiment 43) pr
numbers were as stated, sev
The mother, therefore, of 91
even greater excess of singles
in-bred.

It must also not be forgott

evidence that in-breeding has increased the dominance even on the dominant side, as the cases that at first sight point so clearly in that direction may, perhaps, be capable of another explanation.

On the recessive side the evidence is even more uncertain. It became early apparent that in the case of pure *recessives*, in-breeding did not have that strong influence on the determination of dominance which it may be suggested it had in the case of pure dominants; or rather, if this effect were produced, it was much less marked. As, however, none of the actual relationships between the original White Leghorns were known, it was not at all certain that, when birds of the same strain were mated together, the offspring would be very closely bred even for one generation. Lastly, the only recessive birds yet tried, which are known to be the offspring of brothers and sisters, have not yet shown any certain increase of dominance (Experiments 51, 63, and 66).

Whether or no any of the "prepotency"* yet observed can be ascribed to in-breeding, a question which must still be left open, there is the certain fact that very distinct "prepotency" did appear on the recessive side in many instances (Experiments 43, 50, 54, 56, 62, 68, also in 74, 75, and 77, as compared with 70—73). In two of these cases also there is evidence that the proportions in successive seasons were quite different (*cf.* Experiment 54 with 55, Experiment 56 with 57), but in some others the proportions remained almost identical in successive seasons (*cf.* 70 with 71, 74 with 75). In the former case it should be noticed that in Experiments 55 and 57 the I.G. hens were old, probably in their last season of laying, and though the hens in 75 and 71 were of the same age, yet 3 years of breeding does not in the case of Leghorns nearly finish the period of fertility, and in fact such decline had not begun.

In this connection reference should be made to the interesting result obtained by Ewart† in crossing a pure Turbit with a Jacobin-Barb cross-bred female. Though not on a statistical basis, the facts arrived at distinctly support the view that with the age of the mother, the nature of her gametes changed. It is not, however, certain that this change was merely one of dominance,‡ which is the interpretation Ewart suggests. For in the light of what we now know the *cross-bred* mother must herself have been producing various gametes transmitting various characters, and the difference in the results at different ages may consequently have been due not to a change in dominance, but to the production of gametes carrying different allelomorphs (see p. 138).

A question which has been constantly kept in view is the possibility that in mixed families some proportions may be more common than hers. On the hypothesis that all such mixtures point to impurity the dominant side, it is to be expected that the average of all such ses would show the proportions equal. This expectation as regards th kinds of dominant comb compared with recessive comb is very osely realised, the totals being 459 dominant, 475 recessive. This quality may, nevertheless, have been brought about by many causes, oice of pairs, &c., and cannot be regarded as significant. The ttention of those who are practically engaged in such experiments ay be called to the considerable frequency of cases where equality is pproached, to several cases of 3D : 2R and to several cases of 1D : 2R. he observations are yet far too few to justify any weight being laid n these occurrences, which so far seem fortuitous.

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* This word is used as signifying determination of dominance.

† 'Sci. Trans. Roy. Dub. Soc.,' 1901, vol. 7, p. 360.

‡ *Supplying the* Mendelian term.

Results of Breeding from Members of Mixed-first-cross Families.—On e "first hypothesis," assuming Mendel's law to be strictly followed, e results on using recessive members of mixed-first-cross families hould be the same as when pure recessives were used. The only foot ecessive yet tried did not give this result and therefore is plainly *not* R as regards foot. The comb recessives, however, when bred together r with pure single-combed birds, did appear pure and incapable of ransmitting the dominant character. On the second hypothesis his is the fact most difficult of explanation and is indeed entirely aradoxical.

comb cross-bred gave respectively 38 p.c., 22 s.c., and 46 p.c., and 21 s.c., or together 84 p.c. to 43 s.c., thus approaching 2D : 1R. But a single-comb cross-bred ♀ from the same family mated with the same male (Experiment 82) gave 34 p.c., and 50 s.c. She, however, must be supposed (p. 115) to be a pure RR, and thus we should expect equality in her offspring. Therefore we perceive that the cock was at this time almost certainly producing an excess of recessive germs ; and as the three hens used in 80—82 were all with him at the same time, it may well be this excess of recessives which has caused the numbers in 76 and 77 to depart from 3 : 1.

On the Dorking side in Experiment 85 two dominant first cross hens from Experiment 74 were bred with own dominant brother, giving 131 rose combs to 53 single combs, or 2·47 : 1. Again the recessives are in excess, and the only other evidence as to the cock is the slight indication given by Experiment 87, where there was the expected approximate equality in a total of only 15. Taken together the four comparable totals amount to 215 D : 96 R, or 2·25 : 1. Though the departure from 3 : 1 is here decided, it cannot yet be taken as conclusive against the view that we are here dealing with DR birds, for, as has already been seen, considerable fluctuations are to be expected.

Summary of Conclusions from Experiments with Poultry.

1. When distinct breeds are crossed together certain structural characters in particular strains may exhibit the phenomenon of dominance, the antagonistic character being recessive.

Dominant characters are pea comb, rose comb, extra toe, while single comb and the normal foot are recessive. In the case of each of these pairs of characters, the cross-bred may show some blending, and as regards pea comb crossed with single, or extra toe with the normal foot, the intensity of the dominant character is often considerably reduced. In these cases, it would be possible to recognise the cross-breds *as a rule* from the dominant parents, though sometimes the dominance may be complete. The recessive form is always quite distinct.

2. When the offspring of such crosses are bred together their progeny are mixed, some presenting the dominant, others the recessive character, in proportions following Mendel's law with fair consistency, though in certain cases disturbing factors are to be suspected.

3. When some strains possessing the usually dominant character were crossed with pure recessives, this first cross resulted in a mixture of dominants and recessives. Whether in the case of comb-characters such a mixture may be taken as proving that the dominant parent was itself giving off recessive gametes, or as indicating that the usually *recessive character* may sometimes dominate, there is not yet sufficient

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8. There is no correlation yet p
comb, feet, and colour of plumage

PART III.—THE FACTS OF HEREDITY IN THE LIGHT OF MENDEL'S DISCOVERY.

As was stated in the introduction to this paper, with the discovery of the Mendelian principle the problem of evolution passes into a new phase. It is scarcely possible to overrate the importance of this discovery. Every conception of biology which involves a knowledge of the physiology of reproduction must feel the influence of the new facts, and, in their light, previous ideas of heredity and variation, the nature of specific differences, and all that depends on those ideas must be reconsidered, and in great measure modified.

If we turn to any former description of breeding experiments we generally perceive at once that the whole account must be re-stated in terms of Mendel's hypothesis, and that the discussions and arguments based on former hypotheses are now meaningless. As an illustration we may take the account which Darwin gives of his experiments with peloric Antirrhinum.* He crossed the peloric form with the normal and *vice versâ*. The first crosses were all indistinguishable from the normal or zygomorphic form. These were allowed to fertilise themselves, and gave a crop consisting of 88 normals, 2 intermediates, and 37 perfectly peloric. He discusses these results on the hypothesis that the normal plant has a "tendency" to become peloric, and the peloric a "tendency" to become normal, "so that we have two opposed latent tendencies in the same plants. Now with the crossed Antirrhinums the tendency to produce normal or irregular flowers, like those of the common Snapdragon, prevailed in the first generation; whilst the tendency to pelorism, appearing to gain strength by the intermission of a generation, prevailed to a large extent in the second set of seedlings. How it is possible for a character to gain strength by the intermission of a generation will be considered in the chapter on pangenesis."

Now, of course, we can perceive that the zygomorphic form is dominant and the peloric recessive, and that the arguments based on other hypotheses have no longer any significance. It would be a useful task to go similarly through the literature of breeding and translate the results into Mendelian terms. Such an exercise would show that the change which must now come over the conceptions of biology can only be compared with that which in the study of physical science followed the revelations of modern chemistry.

The outcome of such a revision of current conceptions it is impossible to foresee, but we propose in the present paper to consider some of the more important questions which are immediately raised.

To denote the new conceptions some new terms are needed. Several have already been suggested by Correns, but in practice we have not found his terminology altogether convenient, or that it meets the new conveniently denoted by one w

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* 'Animals and Plants,' vol. 2, p. 46, ed. 1885.

Correns' terms "homöogono be used with precision to mea breeding "true," and, for reaso may be incorrect.

The terms also "*halb-identisc* racters, are already fairly well imperfect correlation, which a be confusing to introduce the m ideas.

But while doubting whether be found adequate, we do not terms for the same phenomena. tions arising from the Mendelia are absolutely required, and f minology.

In the introduction (p. 6) the essential fact discovered by subordinate appearances. We that reasoning, showing how we mena.

By crossing two forms exh breds were produced. The gen shown to be of two kinds, ea parental characters. This nature. We cannot as yet determine the limits of its applicability, and it is possible that many characters may really be allelomorphic, which we now suppose to be "transmissible" in any degree or intensity. On the other hand, it is equally possible that characters found to be allelomorphic in some cases may prove to be non-allelomorphic in others.

It will be of great interest to determine how far the purity of the germ-cells in respect of allelomorphic characters is an absolute rule, or whether there are exceptional cases in which such purity may be impaired. That such exceptions may arise is indeed almost certain from the evidence of "mosaic" fruits in *Datura*, where it was shown (p. 23) that the otherwise pure extracted recessives (thornless) showed exceptionally a thorny patch or segment. Unless this is an original sport on the part of the individual, such a phenomenon may be taken as indicating that the germ-cells may also have been mosaic.*

Indeed all that we know of the occurrence and distribution of variation among repeated parts, would lead us to expect such a possibility with confidence.

This is a question we can analyse no further. Were it possible to do so, it might be a real help towards getting a picture of the actual process of heredity.

But besides the strictly allelomorphic or Mendelian distribution of characters among the gametes (with or without mosaics), we can imagine three other possible arrangements. (1) There may be a substantial discontinuity, the two types of gamete being connected by a certain proportion of intermediates, such as are often met with in cases even of almost complete discontinuity among zygotes. (2) There may be continuous variation among the gametes, shading from gametes pure to the one type, to gametes pure to the other type, the intermediates being the most frequent. (3) There may be no differentiation among the gametes in respect of parental characters at all, each representing the heterozygote characters unresolved. This last is the homoögonous type of Correns. By a sufficiently wide survey, illustrations of each of these systems and of intermediates between them, will doubtless be found, and the classification of gametic differentiation according to these several types, in respect of various characters, in various species, will be a first step towards the construction of a general scheme of heredity.

In gametic variation we thus meet in fact the same series of possibilities with which we have been familiar in the variation of zygotic organisms.

The second fact observed by Mendel is that each heterozygote produces on an average equal numbers of gametes bearing each allelo-

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* Conceivably the cases of poultry having one foot with extra toe and one normal, may be of a *similar* nature, though for various reasons this is unlikely.

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such a case. Readers of Mendel's paper will be aware that he laid down no universal rule as to the absolute purity of gametes, but merely pointed out that his results were explicable on the hypothesis of such purity.

The statistics, however, are not so precise as to compel us to accept *both* that the germs of the cross-breds are *always* pure, and that they are *always* produced on an average in equal numbers.

The next point arising immediately out of Mendel's work concerns the characters of the heterozygote. In the *Pisum* cases the heterozygote normally exhibits only one of the allelomorphs clearly, which is therefore called the dominant. It is, however, clear from what we know of cross-breeding, that such exclusive exhibition of one allelomorph in its totality is by no means a universal phenomenon. Even in the pea it is not the case that the heterozygote always shows the dominant allelomorph as clearly and in the same intensity as the pure dominant, and speaking generally, heterozygotes, though in numerous instances readily referable to one or other of the allelomorphic types, exhibit those types in a more or less modified form.

Besides these, there are undoubtedly cases in which the heterozygote may show *either* of the allelomorphs, though one is commonly dominant. In the poultry crosses it was shown that the usually recessive foot-character (want of extra toe) may appear in the cross-bred. The want of dominance of hoariness in *Matthiola* seen in exceptional cases is a wholly different phenomenon (see pp. 45 and 79).

From the analogy of poultry, it is scarcely doubtful that polydactylism in man is also allelomorphic to the normal, and here from the tables of heredity already recorded,[*] there is good evidence that both the normal and the polydactyle offspring of one polydactyle parent can transmit the polydactylism; in other words, the heterozygote may exhibit either allelomorph. Cases of the same phenomenon can indeed be multiplied. It must, however, be remembered that what is accepted as evidence of alternative inheritance, is not a proof that the dominance of either allelomorph is imperfect. This can only be known for certain when it has already been established that individuals showing either of the two allelomorphs can, when mated with an individual showing the same allelomorph, produce both allelomorphs among their offspring.[†]

This leads to a point of great importance to the evolutionist We

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[*] For examples see Fackenheim, 'Jen. Zt.,' xxii, p. 343.

[†] For the present, therefore, we are not entitled to assume that the numerous cases among Lepidoptera of varieties breeding together with a discontinuous mixed result are allelomorphic, probable as this conclusion is. Such cases are those of *Amphidasys betularia* and *doubledayaria*; *Aglia tau* and *lugens*; *Angerona prunaria* and *sordiata*; *Miana strigilis* and *æthiops*, &c. See Standfuss, 'Handb. d. pal. Gross-Schmetterl.,' 1896, p. 305, *et seq.*

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Another difficulty will be f

1. *Heterozygous Forms.*—It has long been known to breeders that certain forms cannot be fixed by selection indefinitely continued. In other words, when the most perfect examples of such forms are bred together, though they produce some offspring like themselves, they have also a large number which do not resemble them.

A case of this kind is seen in breeding crested canaries. The kind of crest desired for exhibition can, according to canary-fanciers, be produced most easily by mating crested birds with non-crested, or plain-heads as they are called. If it be supposed that the crested character is usually dominant, we have a simple explanation. When crested birds are bred together a number of birds are produced whose crests are coarse and stand up and others without crests. The latter are the recessives; the former we may suppose to be the pure dominants. What the fancier wants is a crest composed of long feathers lying evenly down over the head. These may be the heterozygotes, and consequently cannot breed true or be fixed by selection. Such birds bred together, give many plain-heads and birds with coarse crests. Fanciers hold that the plain-heads needed for crest-breeding should be themselves crest-bred, *i.e.*, from families which have had crests among them. On the view here suggested this is probably a superstition, though one can easily see how it may have arisen.

If two crested birds are bred together it is advised that they should have imperfect crests, in all probability another form of the heterozygote.*

Another case, to which our attention was called by Mr. G. Thorne, of Broxbourne, is that of the Golden Duckwing Game Fowl. This colour can be produced by crossing Black-Reds with Silver Duckwing; but on attempting to breed the Golden Duckwing true, the colour breaks up again into its components.†

Probably the impossibility of fixing certain colours in Pigeons also illustrates the same phenomenon.

Such forms have hitherto been regarded as exhibiting "instability." Of this instability there is now a satisfactory account.

A more complex instance of this may be the Andalusian fowl. The colour is a blue-grey mixed with dull black. The breed will not continue true to colour. Though a considerable proportion of Andalusians are produced, a number will be hatched which are too dark or too light in various ways and proportions. Selecting the best Andalusians effects nothing, and the constancy does not increase. There is, therefore, a strong probability that the Andalusian is a heterozygote,

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* An account of these facts is given in Blakiston, Swaysland, and Wiener's 'Canaries and Cage Birds,' p. 128. When birds with good crests are bred together the recessive "plain-head" is often produced, a fact which has been exaggerated by various writers into the statement that the offspring of cresteds are always plain-heads, or even always *bald*.

† *See also* Lewis *Wright*, 'Book of Poultry,' 1886, pp. 289 and 356.

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2. *Selection and the Phenom*
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The hypothesis that such a "rogue" is a recessive form *may* give a complete explanation of this phenomenon in many cases. Selection from *individuals* of known fertilisation would at once test the truth of this view, and might provide a means of producing a pure strain once and for all from the pure dominants.

It is well known that some of the best modern beardless wheats which have been raised of late years by crossing distinct varieties will give a small proportion of bearded plants. This is, of course, called "reversion" to a bearded ancestor used in the original cross.

From the experiments of Rimpau,* we find that when bearded and beardless varieties are crossed, beardlessness is dominant, and the bearded character is recessive. By subsequent breeding a form is produced with a desirable character, and after a few years of selection it is found to give this character with sufficient purity and it is put on the market. It may be a bearded or a beardless form, but if the latter, the chances are that it will always produce a certain proportion of bearded plants.† This may happen in every case where there has been a *promiscuous* selection of many dominant plants, for any one of these may be a heterozygote and bear in each year both dominant and recessive germs.

The fact that the hornless breeds of goats still give some horned offspring is probably referable to the same cause. The point is of course not certain, but from the analogy of cattle (see p. 140) we may anticipate that the hornless form is dominant. In the polled breeds of cattle, which are never *promiscuously* selected, the polled character has naturally been easily fixed pure, but in goats selection among the *ewes* has been probably to a large extent promiscuous.

The phenomenon is without doubt occurring very widely in nature. To it we may perhaps attribute the undiminished persistence of some weakly varieties, which are unceasingly exterminated by natural or artificial selection without ever leaving offspring. Cases have only to be looked for to be found in abundance. We may note the paradox that, for anything we know to the contrary, a recessive allelomorph may even persist as a gamete *without the corresponding homozygote having ever reached maturity in the history of the species.*‡ It would be premature to trace out the deductions to which this suggestive fact points, but we see at once that it may give the true account of the phenomenon that domesticated forms constantly give rise to varieties not met with in the wild state, a fact often ascribed on insufficient grounds to the action of changed conditions in producing greater *variability*.

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* 'Landw. J. B.,' 20.

† Such a variety is Garton's Red King.

‡ [In illustration of such a phenomenon we may perhaps venture to refer to the extraordinarily interesting evidence lately collected by Garrod regarding the rare condition known as "Alkaptonuria." In such persons the substance, alkapton, forms a regular constituent of the urine, giving it a deep brown colour which becomes black on exposure. The condition is exceedingly rare, and, though met with in several members of the same families, has only once been known to be *directly transmitted* from parent to offspring. Recently, however, Garrod has

It will be clear—a point which may have some economic importance—that in any such case the recessive "rogue" can be eliminated by selection from *individual* plants or animals, breeding only from those which give no recessives on being self-fertilised, if hermaphrodite. If the organism be diœcious the process will be more elaborate, for it will be first necessary to test for recessive allelomorphs by fertilising with a recessive, and afterwards to fertilise those that gave no recessive offspring, with a dominant similarly proved to be free from recessive influence. Nevertheless it is certain that by this process alone can a strain of pure dominants be readily made.

"Purity" then acquires a new and more precise meaning. An organism resulting from an original cross is not necessarily pure when it has been raised by selection from parents similar in appearance for an indefinite number of generations. *It is only pure when it is compounded of gametes bearing identical allelomorphs, and such purity may occur in any individual raised from cross-bred organisms.*

An organism can be strictly defined as genetically pure if all its gametes when united with similar gametes reproduce the parent identically; and in practice the only way in which such purity can, by one breeding, be tested, is by crossing the organism in question with

The cases taken are many-leaved clovers, a polypetalous Ranunculus, several plants with variegated foliage, and the biennial forms of certain species.

Selection in each case at first rapidly increases the proportions in which the selected form appears among the offspring, but soon a maximum effect is produced which is not surpassed.

Now in each of these examples fertilisation was left to insects, and though seed was saved from individual plants it is not in dispute that cross-fertilisation between them occurred. In Mendelian terms some might be pure D, some pure R, and some DR. Supposing dominance complete, eradication of the pure R forms annually does not extinguish them, for by the breeding of the DR forms *inter se* they will be continually reproduced.

There are no doubt many overlying complications in each of these cases, as, for instance, the probability that dominance is in these instances imperfect, but these will not change the main result.

The case of the biennial plants is especially interesting, as here we have strong indications that treatment and conditions may determine which character shall appear. For example, de Vries quotes the evidence of the Sugar Beet, a plant of great economic importance, to the breeding of which much attention has been devoted.

The plant which forms the large sugar-bearing axis is a biennial and does not flower until it has made the sugar-store. But from the best seed which has for generations been saved from such plants only, there arises a small percentage of an annual form which runs to seed without making a thick root at all. After years of selection the proportion of such rogues is not diminished. Now, if it could be supposed that the annual is recessive and the biennial dominant, this is partly explained. On selection, seeds are taken from dominants only. But some of these will be pure dominants and others will be heterozygotes bearing *both* allelomorphs. The latter will each year give rise to a certain number of pure recessives, compounded of two recessive gametes. In the first years of selection, the proportion of recessives will be diminished rapidly by choosing seed from dominants only, but further *promiscuous* selection of dominants, unless continued for an indefinite time, will not altogether remove the recessives, for they arise from the dominants themselves.*

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* It is of course only a conjecture that the biennial form is dominant in these cases, but, owing to the great importance of the subject, it seems worth while to call the attention of those interested to the possibility. Among the many investigations already made on the Beet it does not appear that the simple experiment has been tried of seeing if the annual or biennial form can be bred true from *individual* plants fertilised under proper precautions. Still less has the possibility of dominance been investigated. The only evidence known to us is that of Rimpau, that when the annual *Beta vulgaris* was grown near the cultivated form it bore *two seeds which* proved biennial and fifty-eight which were annuals. Rimpau

But in these forms it is well known that several kinds of treatient, exposure of the young plants to frost, over crowding, heavy ianuring, and forcing, will greatly increase the proportion of "runners." n the case of *Œnothera* de Vries has made some very convincing xperiments, clearly proving this fact, and Rimpau has done the same ɔr the Beet, showing that the number of " runners " can thus be greatly icreased. There are then some which are biennial in any case, some 'hich are biennial or annual according to treatment, and some which re in any case annual. This is strongly suggestive of the three Iendelian classes.

De Vries has also experimented by selection from the annual plants, etting of course a higher proportion of annuals. But it must be emembered that in order to prove that the annual character is ecessive, and that it can, as such, be fixed by one selection, it is ecessary to ascertain first that the plant chosen is not what de 'ries calls a "facultative" annual—on this hypothesis, a DR—and econdly that it has not been cross-fertilised, particulars not yet forthoming.

But even if the hypothesis of dominance could be successfully pplied to these cases, there are others at first sight similar, where it annot be thus applied; for example, instances of varieties recessive in ieir differentiating character, producing annually a small but sensible in the causation of such appearances, and those who are familiar with statistics of inheritance, in man for example, might usefully study them with the possibility in view. The absence of the character in the first generation may indicate merely that it is recessive, and its reappearance in the next generation may be due to the heterozygote having bred with another individual also bearing the recessive allelomorph.

4. "*New*" *characters may be dominant.*—We cannot as yet perceive any properties common to dominant as compared with recessive characters. It will be noted, however, that the view of many naturalists that the phylogenetically older character is prepotent, or, more correctly, dominant, is by no means of universal application. In poultry, for instance, both pea and rose combs are dominant against single, though the latter is almost certainly ancestral; the polydactyle foot is dominant against the normal, though a palpable sport. A point of some interest is that in both wheat and barley the beardless form is dominant, though we naturally, though perhaps incorrectly, regard it as a state normal in the one species, but an innovation in the other.

In cattle the polled form is dominant over the horned, though the former is a character which in our cattle has certainly arisen since domestication.

5. *Prepotency.*—The conception of dominance avoids certain difficulties which are involved in the use of the term "prepotent." As we now know that the allelomorphs of the several characters may be quite independent, it is confusing to speak of the prepotency of an individual when all that we know is that one or more of its characters is dominant over the contrary character. Of the dominance or prepotency of the *whole* we know nothing. The diversity of the views which have been at various times expressed as to the respective powers of mother or father to confer special qualities has probably arisen from confusions thus caused. If the term prepotency is to be preserved it must be applied to characters rather than to organisms, and its use must be restricted to cases in which the character so qualified has been actually tested by combination with the contrary allelomorph in one heterozygote.

We have been accustomed to consider that a variety may be sometimes prepotent in respect of a given character and sometimes not prepotent. The whole evidence on which this view is based will in many cases now require careful verification, for, as was fully discussed in the case of poultry, such a result may really be due to an unsuspected heterozygote having been sometimes used for the other parent. The evidence, for instance, that on crossing pea comb and single comb the offspring may be sometimes pea and sometimes single would formerly have been thought a clear proof that pea comb was not always

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On the other hand, we have more frequent cases of unfertilised eggs in other types producing females only.

But from the observations of de Buzareingues,* it appeared that there is a more or less definite distribution of the sexes among the seeds of diœcious plants, the females being more commonly derived from seeds of one region, and the males from those of another. This of course is no proof of *original* differentiation of sex among the female cells, but it is readily consistent with that hypothesis.

On the other hand, as on the whole *against* the hypothesis that sex depends chiefly on gametic differentiation, may be mentioned observations—especially those of Wichura ('Bastardbefruchtung,' p. 44)—that the statistical distribution of sex among first crosses shows great departure from the normal proportions. The same has been seen by many hybridisers using animal types. But the further fact that there is a still greater variation in the statistical relations of the sexes in the *offspring* of hybrids, is rather favourable to the hypothesis.

The frequent occurrence of hermaphrodites among *first* crosses is also difficult to explain on the present hypothesis.]

7. *Reversions.*—With the Mendelian conception of the heterozygote as a form with its own special "*hybrid character*," we have a *rationale* of large numbers of "reversions"; for we already know many cases where heterozygotes do present the characters of putative ancestors. This fact reduces to harmony several groups of results where different experimenters, believing themselves to have worked with similar organisms, have reached seemingly contradictory conclusions. For some have used pure forms and others heterozygotes appearing in their guise.

THE NATURE OF ALLELOMORPHISM.

A. *Simple Allelomorphs.*

The following list enumerates the principal cases in which the phenomenon of allelomorphism has either been actually proved to exist or may be safely inferred from the published records.† In each of these cases more or less definite dominance of one character has been found, and in this list the dominant character is put first:—

1. Hairiness and absence of hairs (Lychnis).
2. Hoariness and absence of hairs (Matthiola).
3. Felted ears and smooth ears (Wheat).‡

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* 'Ann. Sci. Nat.,' vols. 16, 24, and 30; 1829, &c.

† [From the evidence of crosses kindly carried out for us by Mr. Leonard Sutton we are able to add the "palm" leaf (palmatifid) and reddish stems of *Primula sinensis* as dominant characters, while the "fern" leaf (pinnatifid) and purely green stems are recessive characters.—March, 1902.]

‡ Rimpau, '*Landw. J. B.*,' 20, 1891, p. 346.

experiments seen at M
add tall habit and dwarf
as "Cupids") in Sweet

13. Entire petals and laciniat
14. Normal zygomorphic for
probably Linaria).**
15. Normal habit and waltzi
of the aural labyrinth) (
16. Presence and absence of
17. Pea comb and single com
18. Rose comb and single cor
19. Polled and horned breeds

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* De Vries.

† Rimpau, *loc. cit.*, pp. 341 and
received Tschermak's valuable analy
which considerably extends our know
'Ztsch. für d. Landw. Versuchswesen

‡ Correns, 'Biblioth. Bot.,' vol. 53

§ Some distinct exceptions to the r
known. [See final note.]

De Vries.

¶ Darwin, 'An. and Plts.,' ed. 2, v

** In the case of *Linaria*, Naudin
a normal one a mixture of normal an
of the peloric parent there is no infor
a heterozygote. See Naudin. 'Nouv.

20. White shanks and yellow shanks (Fowl).
21. White plumage and general brown coloration (Fowl).*
22. Several coloured forms of flowers and their white varieties.
23. Several colours of fruits and their xanthic varieties (Atropa, Solanum).
24. Several colours of seed coats.
25. Darker and lighter colours of endosperm (Maize).
26. Yellow and green cotyledons (Pisum).

With regard to seed-colours, Correns has shown that the question is a complex one, depending on several factors. In Maize, especially, the seed-skin and the several parts of the endosperm may all be independently concerned in giving the net result. Each must be considered separately, and in several cases the dominance is imperfect, and blendings may occur.†

Between various simple allelomorphs correlations may of course occur. A few of these we know already. But in these cases of simple

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cross between a pure Angus and a pure Shorthorn is almost always a blue-grey without horns. Generally the horns are represented by loose corns of horny material, sometimes imbedded in the skin and not rarely hidden by the hair. Such "scurs," as they are called in the north, are objected to in the pure polled breeds and are mostly absent.

Notes of the cross-breds exhibited at the Smithfield Club Cattle Shows in 1888, '89, '98–'01 give the following results. The animals are classified according to the descriptions in the Catalogue. No doubt, however, the actual purity of the parent breed or breeds was in many cases doubtful. Taken as they stand, the numbers exhibited in these six years were as follows:—

From $\left\{\begin{array}{l}\text{Polled Angus}\\ \text{Polled Galloway}\\ \text{Red Polled}\end{array}\right\}$ × some horned breed, usually Shorthorn, and the reciprocal cross—104 polled, 13 horned.

From first cross animals bred as above, mated with a pure polled parent—23 polled, 1 horned.

From first cross animals mated with some horned parent—18 horned, 24 polled.

When allowance is made for the very rough materials out of which these figures come, it is clear that the facts cannot be very far from the Mendelian expectation. It is, however, likely that the allelomorphs concerned are not merely the horned character in its entirety, and total absence of horns. For in the offspring of (polled × horned) × polled, the horns, when they occur, are often *loose* though of fair size. If all parts were completely correlated we should expect *either* absence of horns (perhaps mere scurs) *or* ordinary horns like those of horned breeds. Probably, therefore, there is not *complete* correlation between the formation of horns and that of the bony cores which carry them, and these characters are divisible in transmission. Unfortunately the cross-breds are practically never bred together, so the valuable evidence thus attainable is wanting. It should be mentioned that in offspring of (polled × horned) × horned the coat-colour character also breaks up.

* [Wh. Dorking × Ind. Game crosses are this year giving exceptions to dominance of white.—1902.]

† *Full details given* in Correns' memoir, 'Biblioth. Bot.,' 1901.

So far, in all or nearly all the
and recessive characters are
heterozygotes breed together,
like their parents, heterozygote
The gametes therefore respecti
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We have next to consider a nur
which a character of one of the
is itself split up. Of these we

1. *Sweet Pea.*—By the great
have been permitted to watch
their nurseries. We cannot su
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pose now to refer very briefly to

Sweet Pea (Lathyrus odoratus
chocolate, with wings similar
crossed with Giant White, ga
standards as in Stanley, but wi
tilised, gave Giant White, Gian
(a well-known red variety), Her
and a form like Her Majesty, *bu*

One plant of each was saved

The facts point to a higher degree of complexity than we can yet realise, but we see that the first crosses are all alike, though differing from the coloured parent. The same form, or something very like it, was often observed to come in other cases where a blue or purple parent was used in crossing. Now on self-fertilisation the first cross gave a variety of forms. It therefore produced a variety of gametes, not two kinds, but several. Of these forms some, Mars, Her Majesty (Giant White also in all probability), reproduced themselves exactly. Therefore they had only one kind of gamete, and they must be supposed to have been formed by the union of similar gametes. The purples, on the contrary, produced most of the whole series again, showing that they were producing a variety of gametes like the first cross parent itself.

Her Majesty flaked with white, gave some Her Majesty, some White, some Her Majesty flaked white. Therefore the flaked plants are heterozygotes, formed by the union of a Her Majesty gamete with a white gamete.

We are then led to the conclusion that the allelomorph transmitting the coloration of Stanley is *compound*, and that it can be broken up into simpler and possibly component elements. When *similar* elements, thus extracted, combine in fertilisation, they do not split up again on the formation of gametes. The constituents of the compound allelomorphs may perhaps be spoken of as *hypallelomorphs.*

The fact that Stanley did not occur again is another indication that its colour character had been broken up into *more than two* elements.

Another fact which may point in the same direction is that the purple formed on the first cross is different from that which recurs in the next generation. In fact, this Giant Purple Invincible results from the union of the whole compound allelomorph of Stanley with that of Giant White. We may suppose that it does not come again for the reason that the compound allelomorph has been broken up among the gametes borne by the first cross, and that the union of no two of these, or of any of them with white, results in that particular heterozygote form, Giant Purple Invincible. Inasmuch, however, as Giant Purple Invincible, not yet distinguishable from that produced in this cross, is a well-known and stable form, there must *either* be gametes corresponding to it* (or its male and female gametes must be dissimilar and combine in that definite heterozygote, which is most unlikely). Till the experiment has been repeated on a large scale we must not lay much stress on the absence of Purple Invincible after the break up of

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* Similarly from other crosses seen at Messrs. Sutton's it is clear that the form called "Painted Lady" may be another heterozygote form, though the same is one of the oldest and most familiar fixed forms. According to Mr. S. B. Dicks, there is good reason to believe the purple and the Painted Lady forms to be the oldest varieties. '*Report of* Sweet Pea Conference,' 1900.

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times pile (brown and white).
Brown Leghorns the result is
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not simply like their dark gra
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deduction, but that the types
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be unknown till next

of such phenomena from experiments undertaken for other objects. Of these there are a considerable number which leave little doubt that further examination would disclose such a result. We may mention the observations of von Guaita on mice, from which it appeared that the first cross of albino mice with black-and-white Japanese waltzing mice, gave a grey house-mouse resembling in size, colour, and wildness the wild house-mouse.[*] The first crosses bred together gave albinos, grey mice, black and white, grey and white, and black mice (with the waltzing character distributed among them in proportions closely obeying the Mendelian ratio); of these the albinos produced, with one exception, albinos only when bred together. The grey marked with white, bred together, produced no more blacks or black-and-whites; and the blacks and the black and whites bred together gave no more greys, though both descriptions may still give albinos. Facts like these strongly suggest that, with suitable mating, the classes could be shown to consist of the original albino, and a number of forms, some of which would henceforth be pure, while others would be found to be heterozygous.

3. Another case, possibly of the same nature, is that of the Himalayan rabbit, of which an account is given by Darwin.[†]

The literature of pigeon-fancying abounds with information pointing to a similar *rationale* of the colour phenomena there seen. Formerly the recipes given in such treatises as to the methods of mating to be followed for the production of particular colours would have seemed mere nostrums, but now we can see at least the general basis of fact whence they have been derived.

The experiments with stocks described (p. 81) give cases probably also analogous. Several forms crossed together all gave purple for the first cross, which on being self-fertilised gave other colours in addition to those of the pure parental forms and that of the first cross.

This conception of compound allelomorphs is almost the same as that which Mendel himself introduces in speaking of his Phaseolus crosses.[‡] His analysis does not, however, seem to be strictly correct, and the subsequent reasoning is consequently obscure and not altogether valid. He says if the colour of the red Phaseolus be made up of $A_1 + A_2 + \ . \ . \ .$, then on crossing with a white form a, hybrid unions are produced, $A_1a + A_2a + \&c.$

But it is the group $A_1A_2 \ . \ . \ .$ which is allelomorphic to a, and the heterozygote is $A_1A_2 \ . \ . \ . \ a$, and not $A_1a + A_2a + \&c.$ It cannot be

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[*] Haacke, crossing albinos with grey-and-white Japanese waltzing mice, usually obtained the same result, viz., grey mice, but more rarely *black* mice. The latter result must be taken as indicating impurity in one or other parent. Vosseler, quoted by von Guaita, obtained greys only. See Haacke, 'Biol. Cblt.,' vol. 15, 1895, p. 45.

[†] 'An. and Plts.,' I, p. 113.

[‡] *Loc. cit., p. 35.*

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The importance of this reasoning lies in the fact that we can now recognise that these different new forms may be, in their genetic composition, diverse. We are no longer to expect that it is a matter of chance whether each will be able to transmit any of the other forms, but we perceive that this is a question to be determined by actual observation once for all. When such determinations shall have been made on a statistical basis we shall be able to state precisely the numerical proportions which the gametes of the several classes bear to each other, and hence to determine the actual number of constituents of the compound allelomorph and their relationships to each other. This investigation is now merely a matter for precise quantitative analysis.

Remembering that we have no warrant for regarding any hereditary character as depending on a material substance for its transmission, we may, with this proviso, compare a compound character with a double salt, such as an alum, from which one or other of the metals of the base can be dissociated by suitable means, while the compound acid-radicle may be separated in its entirety, or again be decomposed into its several constituents. Though a crude metaphor, such an illustration may serve to explain the great simplification of the physiology of heredity to which the facts now point.

A marked feature in connection with compound allelomorphism* is the frequency with which in such cases one or more of the heterozygotes present what we have reason to regard as ancestral characters. To such "reversion" we referred in speaking of Sweet Pea crosses. The Sweet Peas produced a flower with purple standard and blue wings approaching what we may regard as a primitive Sweet Pea. Several white varieties of Stock produce a purple form; many of the crosses with the "half-hoary" type gave fully hoary heterozygotes. The Albino and Japanese mice produce a grey house-mouse as their heterozygote. *Why* such heterozygotes should show ancestral characters we do not know; but we can now recognise that such "reversions" are heterozygous mixtures and not constant forms. To speak of such reappearances of ancestral characters as a reappearance of the ancestral *form* is entirely misleading. These heterozygotes will not breed true, and *are* ancestral in no real sense. Not only are they heterozygous and in constitution compound, but, as in the Sweet Pea, several different compounds agree in having the same ancestral form as their specific heterozygote.

It is unfortunate that Darwin's own experiments with poultry and pigeons were so complex that it is now impossible to disentangle the results or to use them for the purposes of these deductions. He records the most complicated unions of birds of different breeds, some homo-, some hetero-zygotes, some exhibiting simple and others compound allelomorphs, and in the statement of results the all-important distinctions between the generations and the offspring of the several individual birds are often not observed.

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* There is no reason for supposing such reversion to be absent in all cases of heterozygotes formed by the union of *simple* allelomorphs, but the few clear cases *known seem to be all* cases including compound allelomorphs.

To sum up the phenomena of compound allelomorphism, we may say that the evidence shows that the characters of a pure form when crossed with another may be broken up into component characters or hypallelomorphs, and that the decomposition may take place in various degrees of completeness.

To the variations which thus arise by resolution of compound characters we propose to give the name *Analytical Variations*. There can be no doubt that a very large proportion of the discontinuous variations in colour, at all events, met with both in wild and domesticated species are of this nature. The fact that similar component forms are similarly related to each other and to the type, in various species, thus provides the true account of numerous phenomena of "parallel" variation.

The facts thus grouped, suggest the following questions. Has a given organism a fixed number of unit-characters? Can we rightly conceive of the whole organism as composed of such unit-characters, ever dissimilar, can be split up, the gametes can be formed, and the inability to form gametes may mean that the process of resolution cannot be carried out. In harmony with this suggestion is the well-known experience of hybridisers, that if there is any degree of fertility in the first cross, with subsequent inter-bred generations the fertility may increase.*

Such increase in fertility is generally associated with some greater approximation to one of the parental forms. In terms of our hypothesis, we may conceive this fact as denoting that offspring formed of gametes which have successfully resolved the characters of the heterozygote, and are not bearers of the irresoluble characters, can form their own gametes with less difficulty.

That the sterility of hybrids is generally connected in some way with inability to form germ-cells correctly, especially those of the male, is fairly clear, and there is in some cases actual evidence that this deformity of the pollen grains of hybrids is due to irregularity or imperfection in the processes of division from which they result. It is a common observation that the grains of hybrid pollen are too large or too small, or imperfectly divided from each other.† Such conditions are what we should expect on the hypothesis here suggested.‡

However this may be, it would be of the utmost importance to discover at which of the divisions leading to the production of the gametes, the allelomorphic characters are divided. Correns has pointed out that the evidence of maize proves that in that case the two nuclei of the pollen tube must both be transmitters of the same character, for, in the fruit of the first cross between starch and sugar varieties, those seeds which have sugar endosperms produce pure recessive (sugar) offspring. This fact proves therefore that the nucleus which

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* Focke, 'Pflanzen Mischlinge,' p. 483; Gärtner, 'Bastarderzeugung,' pp. 333 and 373.

† See *e.g.*, Naudin, 'Nouv. Arch. du Mus.,' 1865, I, p. 95, and Wichura, 'Bastardbefruchtung im Pflanzenreich,' 1865, p. 37. Cases are easy to find.

‡ Remarkable observations bearing directly on this question have recently been published by Guyer ('Science,' vol. 11, 1900, p. 248), as to the spermatogenesis in hybrid Pigeons. The species used are not named, and the account is very brief. He states that in both sterile and fertile hybrids much variation in cell-division was seen, inequalities in chromatin distribution were common and multipolar spindles were abundant. In hybrid spermatogonia there were often more than eight (the normal number) large ring chromosomes. Sometimes there were sixteen small rings. In this case they usually located in two spindles, eight to each. Frequently both large and small rings were present. Guyer suggests, though apparently in ignorance of Mendel's work, that this phenomenon may indicate a "tendency in the chromatin of each parent species to retain its individuality." If so, he points out that in cells with two spindles and eight chromosomes, after division, some of the new cells will have chromatin from one parent and some from the other, and the observed "reversion" of the offspring of hybrids *to parent* species "may be due to the persistence of the chromatin of *only one species in* one or both of the germ-cells."

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* Correns inclines to the
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of transmission of a character already existing. It makes no suggestion as to the manner in which such a character came into existence. The facts, however, leave no room for doubt that at least one character of each pair of simple allelomorphs has arisen discontinuously. The fact that the gametes of the cross transmit each member of the pair pure, is as strong an indication as can be desired of the discontinuity between them. From imperfection of the records, however, we cannot point to many cases where we know both that the origin was sudden, and that the characters obey Mendel's law, though no one practically acquainted with these subjects will feel any doubt that if those records were complete, there would be abundant evidence to this effect. A positive example, however, is that of *Chelidonium majus laciniatum*, of which the modern origin is recorded,* and the allelomorphic nature was proved by de Vries.† It is scarcely doubtful that such varieties repeatedly arise. The Cupid Sweet Pea is another (p. 140).

With regard to the compound allelomorphs, it must be determined by further investigation whether they similarly can come into existence in their entirety, or whether they are capable of synthesis. At present, though we can perceive the fact that they are capable of decomposition, we know nothing of the reverse process.

In the cases (p. 142) we have discussed, it is plainly the simple allelomorph that has discontinuously arisen.

While we can hardly doubt that, of each pair of simple allelomorphs, *one* must have come suddenly into existence, we cannot tell whether this fact means that something is *added* to the original organism, or whether, from the first, the appearance of the new character is to be regarded as a *replacement* of the corresponding character. For example, we do not know whether the greenness of the peas is due to an *addition* of something to the whole sum of the yellow pea, or to a *substitution* of something for the yellow character. We may partly understand the physiological nature of the yellowness and the greenness, or to take a clearer case, of the relation of the starch endosperm to the sugar endosperm, but this is as yet no help in elucidating the question. If it shall appear that the process is one of addition, the conception of the characters *splitting* may prove an incorrect one, and some other metaphor must be substituted.

Of special importance in this regard will be the study of cases where three or more characters are capable of mutual replacement. All cases studied so far are examples in which the allelomorphs are in *pairs*, but we know instances where three or more alternative forms of the organism occur, and an investigation of such cases may throw light on this part of the problem.

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* For literature see Korschinsky, Heterogenesis, trans., 'Flora,' Ergänzungsheft, vol. 89, 1901, p. 248.

† '*Ber. deut. Bot. Ges.*,' 1900, p. 87.

Non-Mendelian Cases.—In th
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Such phenomena are—

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1. *Blended Inheritance.*—At first
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hetero-zygous combinations might, on seriation, give so near an approach to a continuous curve, that the purity of the elements would be unsuspected, and their detection practically impossible. Especially would this be the case in a character like stature, which is undoubtedly very sensitive to environmental accidents.

It is, of course, quite possible that the gametes in such cases do in fact vary as continuously as we see the zygotes do, but this cannot yet be affirmed. The great theoretical significance of this question should therefore lead us to suspend judgment for the present.

2. *First Crosses Breeding True.*—With respect to this phenomenon no experiments on a large scale have yet been made. Most examples are recorded in the form that A and B were crossed together and produced a third form, C. The C's were then bred together and some C's were again produced. We hardly ever are told that in this generation *only C's* were produced. Generally, however, we do not even know so much. The cases for example given by Darwin,* are for the most part general statements that certain new and now definite forms, the Swede turnip, for instance, were produced by crossing. Any such case may, therefore, be merely one of the resolution of compound allelomorphs followed by selection of the forms produced by the union of similar component allelomorphs. This, indeed, is probably the true account of most permanent forms produced by crossing.†

There remain, however, a few cases of which Mendel's‡ own crosses among Hieracia are a good example, in which a distinct form, produced by the first cross, has proved able to transmit its characters to its offspring. Of such cases we know very little. We may, perhaps, notice two features as apparently characteristic of these cases. First, that the results of the first cross may show no uniformity; secondly, that there is often a considerable degree of sterility.

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* 'An. and Plts.,' ed. 2, vol. 2, pp. 73–77.

† We cannot avoid expressing a doubt whether the wonderful series of "mutations" which de Vries has lately recorded ('Die Mutationstheorie,' 1901) as arising from *Œnothera Lamarckiana* do not fall under suspicion that they may owe their origin to some unsuspected original cross. Nothing can take away the extraordinary interest which attaches to these experiments, but until it has been shown in the clearest way that the *Lamarckiana* which gave rise to the "mutants" is a genuine uncrossed form we must feel hesitation in accepting the conclusion which de Vries has drawn from the facts.

This possibility is strengthened by the fact which Professor de Vries has told us, that the pollen of his *Lamarckiana* contains deformed grains, a point which is also mentioned by Pohl (' Oesterr. Bot. Ztschr., 1895, vol. 45, p. 212) in a paper to which de Vries refers (*loc. cit.*, p. 153).

On the other hand, we can scarcely suppose crossing to be the only cause determining the production of heterogeneous gametes, or in other words, variation in sexual descent.

‡ Mendel, 'Abh. Ver. Brünn.,' vol. 8, 1869. See also Swingle and Webber, '*Year-book Dept. Agric.*,' 1897, p. 393.

are crossed together, (1) the maternal type, without any i in other cases the cross-bred i characters pure (save in one characters pure. Seeds from parental type themselves gav To such forms he gives the *croisement.*"

In order to estimate the know of what variations the p without crossing. Upon thi we assume that each of the for characters regularly to its off that the heterozygote produce character which appeared as " facts is thus revealed. It is c clusion, but, on the other hand offspring of the one form co the other parent used in the c forms themselves produced n should expect that when simi spring resulting would breed very improbable, but as yet it cases in which Millardet's phe

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[Note, added March, 1902.

Several times in the course of these pages, reference has been made to the phenomenon known as the "false hybridism" of Millardet. We are not aware that attempt has yet been made to elucidate that phenomenon. In view of the Mendelian discovery, we think it may not be althogether premature to suggest a possibility, which may perhaps be some guide to further experiment with this phenomenon.

In the false hybrid then, one or more characters are contributed to the zygote by one parent alone, to the exclusion of the corresponding character of the other parent. This exclusive character is exhibited on the development of the zygote; and that the opposite character is really excluded appears from the fact that the offspring of the "false hybrid" do not reproduce the excluded character.

The terms "false hybridism" and "false hybrid," though they have done good service, are clearly inconvenient for the fuller discussion that must arise respecting these facts, and we propose to denote the phenomenon by use of the term *monolepsis,* the ordinary result of fertilisation being distinguished as *amphilepsis.*

It is not yet certain whether monolepsis is a phenomenon peculiar to recessive characters; but while we are fairly sure that some of the cases in which it is seen are instances of recessive characters, we know no certain example of the monoleptic transmission of a dominant character. By the nature of the case, positive evidence of such transmission must be peculiarly difficult to obtain; for the first cross-bred generation would have to be individually tested on a considerable scale by subsequent breeding before such a possibility could be established.

Let us first consider certain features of the process of fertilisation as it may be supposed to occur between gametes bearing similar allelomorphs—for example, an R character. Each gamete bears R, the zygote exhibits it, and the gametes produced by that zygote bear it again.

But we note that we do not *know* whether the character exhibited by such zygote is really the product of the allelomorph of *both* gametes, or is due to the exclusive development of that of one gamete only. Commonly we conceive of all characters of a zygote as the product of both gametes, and in cases of true blended inheritance we must so conceive them. Such a view also accords well with all that we know of the visible processes of fertilisation. Nevertheless, the fact is not certain in the case of the union of similar gametes, and the case may —to take a rough and partially incorrect illustration—be comparable to the known fact that the faculty of speech is, in the normal case, controlled by the centre in the left hemisphere only, the corresponding structures presumed to exist in the right hemisphere not developing o at least not becoming functional. We do not *know*, in fact, whether the character in the zygote depends on, or is in any way affected by, the fact that *both* gametes were bearers of that character.

But if we suppose that the zygote character is thus a product of the two similar allelomorphs in the normal case, we may on that hypothesis form a conception of what may be imagined to take place in the case of monolepsis. For returning to the heterozygote we perceive that on the formation of its gametes there is a resolution or separation of the two dissimilar allelomorphs which came into it at fertilisation. May we not then suppose that in the case of the homozygote a similar separation takes place? The gametes of the heterozygote DR are bearers of D and R respectively separated out of DR; may not the gametes of the homozygote, which are bearers of R and R, receive those allelomorphs by a similar separation occurring between R and R?

If this reasoning prove valid, we suggest the possibility that in the case of false hybridisation the allelomorph is passed on from the zygote to the gamete without such resolution, and that thus it is not in a state which admits of its being affected by the contrary allelomorph of the other gamete. The case may perhaps be compared with the known fact that on separating the two segmentation spheres of an egg, each half may develop into a symmetrical larva.

Unproved as such a suggestion must necessarily be, it is in accord with one should begin with a strain of Canaries which, on mating with the Goldfinch, throw some pied birds. On the hypothesis here suggested, the pied character is supposed to be due to the partially unresolved character of the recessive allelomorph. On in-breeding we may conceive the process of non-resolution on formation of gametes to be carried further. We have seen that cross-breeding leads to the fuller resolution of characters—in-breeding may lead to the contrary result.

With the Canary, as the mule is almost (if not quite) universally sterile, further experiment is impossible, but other cases are available for the experimental testing of this hypothesis.

If it is correct, it should appear that when on crossing a D and R a pied form is produced, showing patches of the R character, then such a pied form on crossing with the dominant again is more likely to give pied recessive or recessive offspring than a pure normal recessive would be, for we are on the hypothesis entitled to believe the gametes of the pied mule to partake of the same character as the zygote itself.

On the older view of breeding such a fact would be paradoxical; for the pied form, inasmuch as it already is part way to the D form, would be supposed *less* likely to show any R in its hybrid than the pure R form.

The fact that Tschermak in his crosses between the pea *Telephone* and yellow varieties obtained a considerable number of seeds greenish or patched with green, is consistent with this view; for this pea, though commonly a green or greenish pea, is liable to great variation, and is frequently mosaic or pied yellow and green.*

The remarkable series of Orchid crosses given by Hurst,† in which the female parent's characters alone appeared as the result of certain extreme crosses, seem rather to illustrate the possibility of parthenogenesis following the stimulus of fertilisation, without zygotic union.]

Galton's Law of Ancestral Heredity in relation to the new Facts.

Such a preliminary survey of the phenomena of heredity as we have attempted would be incomplete without some reference to this subject. We note at once that the Mendelian conception of heredity effected by *pure* gametes representing definite allelomorphs is quite irreconcilable with Galton's conception in which *every* ancestor is brought to account in reckoning the probable constitution of every descendant. With respect of each allelomorphic pair of characters we now see that only four kinds of zygotes can exist, the pure forms of each character, and the two reciprocal heterozygotes. On Galton's view the number of kinds is indefinite.

At first sight it may appear that as the two views are quite incompatible, they must relate to different classes of phenomena. In so far as

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* See Weldon, 'Biometrika,' I, 1902, Pt. II.

† 'J. R. Hort. Soc.,' xxiv, 1900, pp. 104-5.

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By any practical breeder th

character may be fixed at once by selecting recessives, and that this fixity may have nothing to do with the novelty of the character, its "prepotency," &c., and that the heterozygote may never come true.

Galton's law in fact does not recognise that *absolute* purity which is so common a phenomenon in breeding, as it is in nature. The breeder, in hosts of instances, is not, as a matter of fact, constantly troubled by recurrences of forms with which, even in his own practice, his strain has been crossed. Of this the full explanation is now seen; for if two similar gametes meet, their offspring will be no more likely to show the other allelomorph than if no cross had ever taken place.

CONCLUSION.

We have now sketched the principal deductions already attained by the study of cross-breeding, and we have pointed out some of the results now attainable by that method. The lines on which such experiments can be profitably undertaken are now clear and a wide field of research is open.

The properties of each character in each organism have, as regards heredity and variation to be separately investigated, and, for the present, generalisation in regard to those properties must be foregone. The outlook, in fact, is not very different from that which opened in chemistry when definiteness began to be perceived in the laws of chemical combination. It is reasonable to infer that a science of Stoechiometry will now be created for living things, a science which shall provide an analysis, and an exact determination of their constituents. The units with which that science must deal, we may speak of, for the present, as character-units, the sensible manifestations of physiological units of as yet unknown nature. As the chemist studies the properties of each chemical substance, so must the properties of organisms be studied and their composition determined.

To the solution of the practical problems of heredity, and a determination of the laws of breeding both plants and animals, this is the first step. The attainment of these solutions is now only a question of time and patience.

That the same method will give the key to the nature of specific differences, we may perhaps fairly hope. Certain it is that until the several characters are thus disentangled and their variations classified, no real progress with this question can be expected.*

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* It is absolutely necessary that in work of this description some uniform notation of generations should be adopted. Great confusion is created by the use of merely descriptive terms, such as "first generation," "second generation of hybrids," &c., and it is clear that even to the understanding of the comparatively simple cases with which Mendel dealt, the want of some such system has led to difficulty. In the present paper we have followed the usual modes of expre

[Since this Report was written, a paper has appeared by Professor Weldon, entitled "Mendel's Laws of Alternative Inheritance in Peas,"* questioning the importance of Mendel's discovery. This paper will be dealt with in a separate publication by W. Bateson, entitled "Mendel's Principles of Heredity," with which is incorporated a translation of Mendel's papers.—March 1902.]

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sion, but in future we propose to use a system of notation modelled on that used by Galton in 'Hereditary Genius.' We suggest as a convenient designation for the parental generation the letter P. In crossing, the P generation are the pure forms. The offspring of the first cross are the first filial generation F. Subsequent filial generations may be denoted by F_2, F_3, &c. Similarly, starting from any subject-individual, P_2 is the grandparental, P_3 the great-grandparental generation, and so on. We suggest this terminology here for the consideration of others who are working in the same field. All that is essential is to obtain uniformity, and it is quite likely that a better system may be suggested.

* 'Biometrika,' I, 1902, Pt. II.

[*Note to page* 140, "Cupid Sweet Pea."—Mr. Hurst called our attention to a passage in 'Report of the Sweet Pea Bicentenary Celebration, 1900' (published by Mr. R. Dean), p. 26, where it is stated that Mr. Laxton, of Bedford, crossed Cupids and tall forms, producing almost all *talls* on the first cross. Inquiry from Messrs. Laxton elicited the fact that in this Report the facts were by mistake inverted.]

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